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Journal

of

THE ROYAL AERONAUTICAL SOCIETY

AUGUST 1958

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CHESTER BRANCH AERONAUTICAL SHOW AND GARDEN PARTY, 1958

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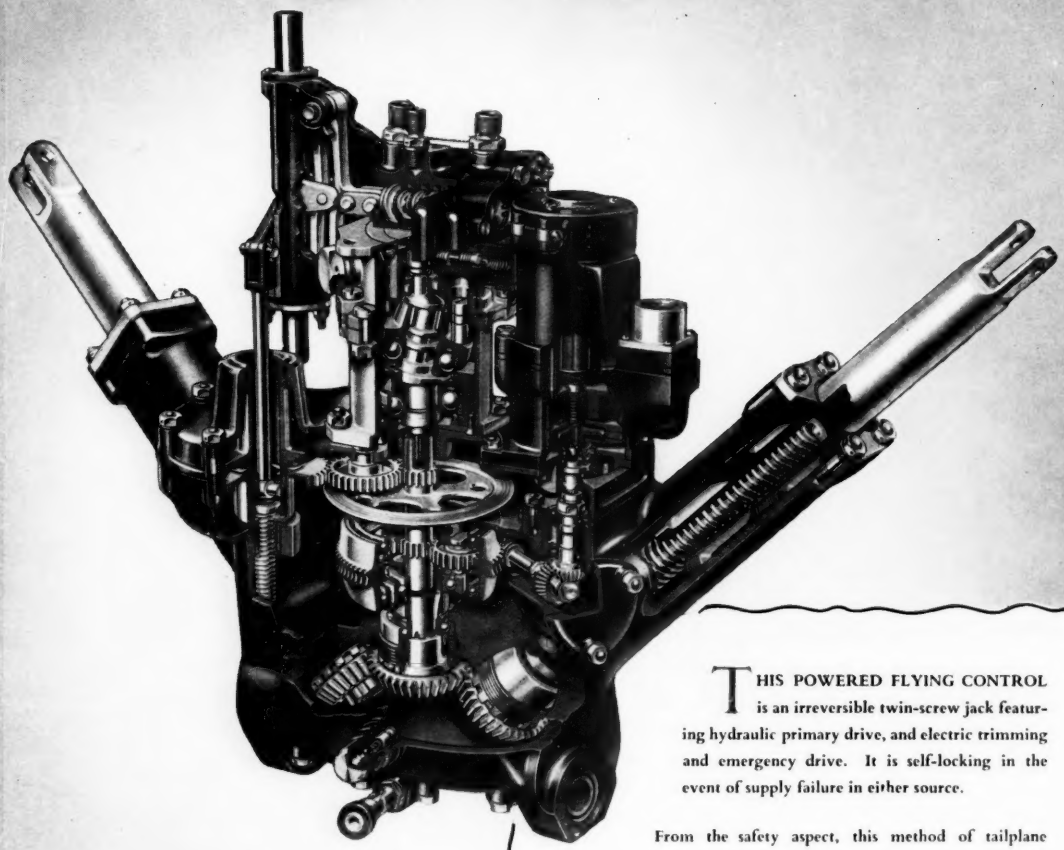

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All communications for publication in, and advertisements for, the JOURNAL *should be addressed to the Editor,* JOAN BRADBROOKE, *A.R.Ae.S.; all communications on general matters affecting the Society should be addressed to the Secretary,* A. M. BALLANTYNE, *T.D., B.Sc., Ph.D., Hon.F.C.A.I., A.F.I.A.S., F.R.Ae.S.*

None of the papers or paragraphs must be taken as expressing the opinion of the Council of the Royal Aeronautical Society unless otherwise stated.

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(Founded 1866)

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NOTICES

Australian and New Zealand Divisions

Mr. M. M. Waghorn, *Associate Fellow*, has been elected President of the Australian Division in succession to Mr. L. P. Coombes, *Fellow*.

Mr. R. E. Hardingham, *Fellow*, Secretary and Chief Executive of the Air Registration Board, gave talks on Airworthiness to the Australian Division in Sydney and in Melbourne. On his visit to New Zealand he addressed members in Wellington and Auckland.

Southern Africa Division

We have heard from the Southern Africa Division that The Hon. B. J. Schoeman, Minister of the Union of South Africa, has kindly accepted their invitation to him to become Honorary President of the Division.

Mr. I. H. G. Nicholson, *Associate Fellow*, remains in office as the President of the Division.

Awards to Members

Mr. P. G. Masefield, *Fellow*, President-Elect, was recently presented with the Paul Tissandier Diploma awarded to him by the Federation Aeronautique Internationale for his work for light aircraft, particularly as President of the Popular Flying Association.

Flight Lieut. E. Rowbotham, R.A.F. (Retd.), *Associate*, has received one of the United States Armed Forces highest awards to be made to a civilian—the United States Air Force Meritorious Civilian Service Award. The Award was presented to him at a ceremony at Burtonwood on 19th June 1958.

Representatives of the Society on other Bodies

Dr. A. E. Russell, *Fellow*, was recently appointed the Society's Representative on the College Board of Governors of the College of Technology, Bristol.

Mr. E. D. Keen, *Fellow*, was recently appointed the Society's Representative on the Advisory Committee of Aeronautical Engineering of the Loughborough College of Technology.

AGARD Structures and Materials Panel

The next meeting of the panel will take place in Copenhagen and sessions will be open to accredited observers from 20th to 23rd October 1958.

The major part of the lecture sessions will be on structures for high speed flight and the programme also allows for panel members and observers to attend some of the lectures organised by the Wind Tunnel Panel and by the Flight Test Panel of AGARD. This meeting takes place before the General Assembly of AGARD to be held in Copenhagen from 27th to 29th October, at which a structural paper will also be presented. Details of the programme are available from A. J. Barrett, Royal Aeronautical Society.

DIARY

BRANCHES

15th September

Halton.—Physiological Aspects of High Speed Flight. Sq. Ldr. T. C. D. Whiteside. Branch Hut, R.A.F. Halton. 6.45 p.m.

17th September

Coventry.—Design of Pattern Equipment for an Aero Engine Casting. K. Marston. Wine Lodge, Coventry. 7.30 p.m.

22nd September

Henlow.—Annual General Meeting and Film Show. Building 62, R.A.F. Technical College, Henlow. 7.30 p.m.

30th September

London Airport.—The Boeing 707. M. Pennell (Boeing Airplane Co.). B.O.A.C. Headquarters, L.A.P. 6 p.m.

6th October

Henlow.—Mixed Power Plants. M. J. Brennan. Building 62, R.A.F. Technical College, Henlow. 7.30 p.m.

Visits to London Airport

Branch Secretaries wishing to arrange visits to London Airport are asked to deal with the appropriate person, as listed below, who will give full information about dates, times, transport and catering facilities.

For B.O.A.C. Buildings, Aircraft, Airport general and Air Traffic Control: Mr. J. R. Wardrop, Assistant, Special Duties, B.O.A.C., London Airport.

For B.E.A. Buildings, Aircraft and Airport general: Works Manager Engineering Base (Attn. Mr. H. A. Smith), B.E.A., London Airport.

For Air Traffic Control only: Mr. G. Housego, Public Relations Officer, M.T.C.A., Central Area, London Airport.

Vacation Course in Meteorology

Arrangements have been made by the Joint Committee of the Field Studies Council and the Royal Meteorological Society for a short vacation course in meteorology to be held at Malham Tarn Field Centre, from 20th to 27th August 1958. The main emphasis will be on all practical aspects of meteorology and previous experience is not essential. The inclusive fee for the course is £8 8s. 0d. Application forms and further particulars may be obtained from the Warden, Malham Tarn Field Centre, near Settle, Yorks.

News of Members

Dr. David Abir (*Associate Fellow*), formerly at the Polytechnic, Brooklyn, U.S.A., has been appointed Senior Lecturer in Applied Mechanics at the Technicon, Israel Institute of Technology, Haifa.

M. Armstrong (*Graduate*), formerly Structural Test Engineer, Vickers-Armstrongs (Aircraft) Ltd., Winchester, is now a Scientist with British Oxygen Research and Development Limited.

G. T. Beauchamp (*Associate*), formerly Design Engineer, Lockheed Aircraft Corporation, Burbank, is now Design Engineer at Lockheed Missiles Systems Division, Palo Alto, California.

Group Captain A. A. Buchanan (*Associate Fellow*) has been transferred to R.C.A.F. Headquarters in Ottawa, where he will be Director of Maintenance Engineering.

J. W. Child (*Graduate*), formerly a Stressman at de Havilland Aircraft Co. Ltd., Christchurch, is now a Designer-Draughtsman in the Aircraft Division of Lockheed Hydraulic Brake Co. Ltd.

J. Ellis (*Graduate*) has resigned his post as a Scientific Officer at the Ministry of Supply and is now an Assistant Engineer (Mathematician) in the Research and Development Department of the Central Electricity Generating Board, London.

Lieut.-General Sir John Evetts (*Companion*), formerly Managing Director of Rotol and British Messier Ltd., has been appointed Chairman with effect from 1st July 1958.

J. S. Gibb (*Associate Fellow*), formerly Inspector-in-Charge, Vickers-Armstrongs (Aircraft) Ltd., Weybridge, now holds the same position with the de Havilland Aircraft Company Ltd., Hatfield.—(Correction from July Notices).

J. M. Gray (*Associate*), formerly Design Draughtsman at Vickers-Armstrongs (Aircraft) Ltd., at Swindon, is now an Assistant Development Engineer with Sir W. G. Armstrong Whitworth (Aircraft) Ltd.

S. A. E. Griffiths (*Associate*), formerly Maintenance Officer, B.O.A.C., is now an Engineer Officer (Air Crew), Britannia 102 Flight.

W. G. Hemming (*Graduate*), formerly Stress Engineer in the London Design Office of Bristol Aircraft Ltd., is now Stress Engineer at Bristol.

A. S. Hill (*Associate*) has been transferred from M.A.E.E. at Felixstowe on its closure, to R.A.E. Bedford in the Inspection Department.

Squadron Leader F. A. HOUGHTON (*Associate Fellow*), formerly at R.A.F. Seletar, is now Senior Engineering Officer at R.A.F. Station, St. Mawgan.

D. HOWE (*Associate Fellow*), formerly Lecturer in Structural and Mechanical Testing at the College of Aeronautics, Cranfield, is now Lecturer in Project Design.

Flight Lieut. J. JEPSON (*Associate Fellow*), formerly Guided Weapons Safety Officer, M.O.S., was posted in May this year to the Central Servicing Development Establishment, R.A.F., Swanton Morley.

J. O. KANE (*Associate Fellow*) has left Blackburn and General Aircraft Ltd. (London Design Office) to join Convair, San Diego, as Senior Engineering Designer.

J. R. LINGE (*Associate Fellow*), formerly Group Leader, Atomic Power Construction Co. Ltd., Heston, has been appointed Lecturer in Mechanical Engineering at King's College, University of Durham.

K. R. LIVERSAGE (*Graduate*) has left de Havilland Aircraft Co. Ltd. and joined the Project and Development Branch of British European Airways Corporation as a Development Engineer, Airframes.

C. J. LUBY (*Associate Fellow*), formerly Assistant Managing Director of Rotol Ltd., has been appointed Managing Director of Rotol and British Messier Ltd.

T. D. MARTIN (*Associate Fellow*) is now employed as a Mechanical Engineer on the design and development of Vending Machines with Ideal Venders Ltd., Ontario.

G. C. R. MATHIESON (*Associate Fellow*), formerly Chief Designer, Engine Division, Blackburn and General Aircraft Ltd., is now Special Assistant to the Technical Director of Richardsons Westgarth (Hartlepool) Limited.

D. P. MOAKES (*Student*) is now a Technical Engineer in the Project Group of Bristol Aircraft Ltd. (Correction from July Notices.)

E. H. J. PALLETT (*Associate*) has been appointed Technical Instructor in the Fokker "Friendship" Service School at Schipol Airport, Amsterdam.

T. PASTERNAK (*Associate Fellow*) is now a Development Engineer in the Research and Development Dept. of the Jet Division of Thompson Products Corp., Cleveland, Ohio.

Squadron Leader G. PIERCE (*Associate Fellow*), formerly at R.A.F., Tengah, Singapore, is now in the Directorate of Aircraft Engineering, Air Ministry, London.

H. S. C. RIGBY (*Associate*), of the Ministry of Transport and Civil Aviation, has been temporarily released to join I.C.A.O. as a Senior Air Traffic Services Expert and Chief of the I.C.A.O. Assistant Mission to Pakistan.

R. G. ROUGH (*Student*), formerly with the Fairey Aviation Co. Ltd. as a Development Technician, is now doing his National Service and has been commissioned in the Corps of Royal Electrical and Mechanical Engineers.

Flight Lieut. R. W. R. SHEPHARD (*Associate*) is now Petroleum Quality Officer, Far East Air Force, Singapore.

K. R. SHERHOD (*Graduate*) has transferred from the Lockheed Aircraft Corporation's Georgia Division to the Lockheed Missile Systems Division, Sunnyvale, California, where he is employed as a Design Engineer "A."

W. J. SELLEN (*Associate*) is now Engineering Superintendent, Planning, with the Sperry Gyroscope Co. Ltd.—(Correction from June JOURNAL Notices).

Group Captain EDWIN SHIPLEY (*Associate Fellow*), Consulting Engineer, has been appointed Engineer Technical Officer to the British Association Metric Committee which was formed recently to investigate the adoption of the Metric System of Weights and Measures in this country or the decimilisation of the Imperial System.

A. W. J. SMITH (*Associate*), formerly of the Air Registration Board, is now Assistant Quality Engineer, Aero Gas Turbine Division, D. Napier and Son Ltd., Acton.

VERNON L. SMITH (*Associate Fellow*) has changed his post with the Lockheed Company from Sales Engineer, Georgia Division, to Research Engineer, Lockheed Missile Division, Santa Cruz Test Base, California.

BIRTHDAY HONOURS LIST

Group Captain S. W. LANE (*Associate Fellow*) received the C.B.E. in the Birthday Honours and not the C.B. as stated in the July issue of the Journal.

ELECTIONS

The following is a list of new members and transfers of membership of the Society:—

Associate Fellows

Geoffrey Charles George Agg (*from Graduate*)
Percy Geoffrey Ball (*from Associate*)
Geoffrey Thomas Bannister
William Henry Barber (*from Graduate*)
Kenneth Bolton
Catherine Grace Joan Bruce
Dennis James Carey (*from Graduate*)
Reginald Leonard Cater
Cyril Hooper Edgecombe (*from Associate*)
Norman Elliott (*from Associate*)
Harry James Evans
Frank William Fahy
Walter Vincent Holden
Sidney Benjamin Jackson
Jacek Kundycki (*from Graduate*)
Donald Roy Lewis (*from Graduate*)
Glenn Kenneth Norman Lloyd
Hugo Marom
Gregory Earle Masterson (*from Graduate*)
Peter Meiklem (*from Graduate*)
Donald George Merchant (*from Graduate*)
Franciszek Misztal
Edgar Phillips Peregrine
Eric Richardson (*from Graduate*)
Geoffrey Edward Rogerson (*from Graduate*)
Robert Rose (*from Graduate*)
Bryan Charles Smith (*from Graduate*)
Leo Sydney William Smyth (*from Associate*)
Peter Henry Southwell (*from Graduate*)
Joseph Carey Crabtree Taylor (*from Associate*)
John Manners Furzan Taylor
Ernest Cyril Thomasson (*from Graduate*)
Keith Donald Thomson (*from Graduate*)
Alan Sydney Thurley (*from Graduate*)
Norman Turner (*from Graduate*)
Francis Ian Victor Walker (*from Graduate*)
Thomas George Webb

Associates

John Leslie Adams
Peter Frederick MacDonald Davies
Richard Dickinson
Donald Francis Dyson
John Stanley Fowler
George Garrington
Philip Thomas Hutchings
Mirza Ahmad Osman Jan
Philip Herbert Johns
Norman Edgar Kasper
Ronald Albert Kirkin
Kenneth McCanlis
Raymond Hackney Millhouse
Maxwell John Ryan
Albert Oliver Simpson
Peter Antony Eabry Stewart
Robert Frank Studdert
Arthur James Whittemore

Graduates

Peter Brighton
Ralph Moseley Eden
Dennis Gerald Cave Goodhead (*from Student*)
John Stuart Haslam
David Michael Kenneth Houston (*from Student*)
Adam Stanislaw Marja Malhomme de la Roche (*from Student*)
Anthony Newman Martin (*from Student*)
Chintalapati Surya Narayana Raju
Keith Leslie Ryder
James William Schofield
Euan Caldwell Semple
David John Sledge (*from Student*)
Malcolm Adam Wallis

Students

Jackson George Henderson Aitken
Anthony Andrews
John David Andrews
Wilfred Arthur Blount
Robert Henry Burrin
Arcot Sivashankeran Chakrapany
Leslie Collier
John Cyril Corby
Gerald Michael Davey
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Michael John Goodyer
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Brian Humble
William Harris Jenkins
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David George Land
Alan Robert Noden
Reginaldo Vittorio Rodosli
Colin Rowland Sabine
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Peter Arnot Smith
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Neil Royston Thomas
Anthony Hugh Whitmore
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The Journal of the Royal Aeronautical Society
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VOLUME 62 AUGUST 1958 NUMBER 572

Flight Tests of a Meteor Aeroplane Fitted with Jet Deflection

by

P. F. ASHWOOD, A.F.R.Ae.S. and D. LEAN, A.F.R.Ae.S.
(National Gas Turbine Establishment and Royal Aircraft Establishment, respectively)

The 31st Main Lecture of the Society to be given at a Branch, "Flight Tests of a Meteor Aeroplane Fitted with Jet Deflection," by P. F. Ashwood, A.F.R.Ae.S., of the National Gas Turbine Establishment, and D. Lean, A.F.R.Ae.S., of the Royal Aircraft Establishment, Farnborough, was held under the auspices of the Christchurch Branch of the Society at the Municipal College, Bournemouth, on 13th March 1958. Mr. G. B. Evans, Chairman of the Branch, opened the meeting by welcoming the visitors and thanking the Airspeed Division of de Havilland Aircraft Co. Ltd. for their assistance in Branch meetings.

Mr. W. A. Tamblin, F.R.Ae.S., President of the Branch, expressed his pleasure at being present on this important occasion, their first Main Lecture at Christchurch. The Branch had been founded two years ago with about 160 members and they now had 300. They were fortunate in having in the area outposts of three of the large aircraft firms, the M.T.C.A. airport at Hurn, Sir Alan Cobham's company at Tarrant Rushton, the University of Southampton, with its Department of Aeronautical Engineering, and the Bournemouth Municipal College. He hoped they would enable the Branch to gain momentum and do justice to its affiliation with the Main Society.

On behalf of the Christchurch Branch, Mr. Tamblin welcomed the visitors to the meeting and particularly Mr. P. G. Masefield, M.A.(Eng.), Hon.F.I.A.S., F.R.Ae.S., Vice-President of the Society, who would preside during the lecture, and Dr. A. M. Ballantyne, T.D., B.Sc., Ph.D., Hon.F.C.A.I., A.F.I.A.S., F.R.Ae.S., Secretary of the Society.

Mr. P. G. Masefield first conveyed the regrets of the President, Sir George Edwards, C.B.E., F.R.Ae.S., at being unable to be present because of his absence in Australia and then his own pleasure at taking the chair at the first Main Society Lecture in the district. The policy of the Royal Aeronautical Society, as they knew, was to have some of the Main Lectures of the Society in what one might call the "outposts of the Empire." He thought it appropriate that the Lecture that night should be on a subject that had an important bearing on work on future aircraft.

They had two lecturers and their lecture would be a story of achievement. Mr. P. F. Ashwood would start the lecture and be followed by Mr. D. Lean. Mr. Ashwood was educated at Whitgift School and the City and Guilds College, University of London, graduating with first class Honours in mechanical engineering. After a year's post-graduate study in the Department of Aeronautics, Imperial College, he joined the Turbine Division of the R.A.E. In 1944 he joined the National Gas Turbine Establishment and had been engaged for some years on the special problems of engine aerodynamics, including work on supersonic propelling nozzles, the cooling of high temperature engines and lifting engines for vertical take-off. He was now head of the Gas Dynamics Department, N.G.T.E.

Mr. Lean graduated from the University College of the South West, Exeter, with first class Honours in physics in 1939, and joined the Royal Aircraft Establishment. He joined the Flight Division of the Aerodynamics Department in 1943, and had worked mostly on problems of low speed flight. He was now P.S.O., leading a group on research on lift augmentation in vertical take-off and landing and general low speed problems of high speed aircraft.

Foreword

The idea of using engine power directly to provide the lift required to sustain flight has recurred frequently throughout the history of aviation. Most of these proposals have concerned some form of helicopter, but in a few cases methods of changing the line of action of the normal propulsive thrust to assist the landing and take-off manoeuvres have been suggested. Few, if any, of these schemes were ever successfully put into practice, one of the main difficulties being the excessive weight and bulk of the power plant, but with the development of the jet engine with its compactness and low specific weight this objection largely disappeared and the idea was brought nearer the realm of practical realisation.

As a direct result of this changed situation, but prompted also by a desire to prevent approach speeds rising continually with increase of maximum speed, early in 1952, there was a revival of interest in schemes for reducing the load to be supported by the wing, particularly during landing, by means of an engine-generated upthrust. It is an interesting commentary on the low specific weight that can be achieved with turbo-jets that the use of separate engines solely for providing this lifting thrust can even be considered, but this certainly is so, at least for aircraft for some specialised duties. However, in the present paper we shall only be concerned with a system in which the thrust of the main propulsive engines is deflected to provide lift.

When the practical problems raised by the use of jet deflection came to be considered, it was felt that many were of sufficient novelty to warrant their investigation on a research aircraft and accordingly it was agreed that a suitable aircraft should be modified to enable the principle to be thoroughly investigated in flight. The main purpose of this experiment would be to study problems of stability and control at speeds below normal stalling speed, but it was also considered desirable to demonstrate that appreciable reductions in approach speed could be successfully obtained in practice.

Several aircraft types were considered as regards their suitability for conversion and the one finally chosen was the Gloster Meteor. Even with this aeroplane extensive modifications to the airframe were necessary to adapt it to jet deflection and these were

undertaken by Westland Aircraft Ltd. Responsibility for the design, construction and development of the jet deflectors rested with the National Gas Turbine Establishment, while the detailed flight testing was done at the Royal Aircraft Establishment.

This paper deals with the two major aspects of the investigation: the development of the jet deflectors and the results of the flight tests.

PART I—DEVELOPMENT OF THE JET DEFLECTOR

1. Introduction

Since the thrust of a jet engine arises from the momentum change imparted to the air flowing through it, there are only two ways in which the line of action of the thrust can be changed: by tilting the entire engine or by altering the direction of the propelling jet with some form of deflector. There are several obvious ways in which this can be done by mechanical devices, for example, by placing deflector vanes in the gas stream leaving the propelling nozzle, by the use of a swivelling jet pipe or by the provision of an alternative nozzle pointing along the direction in which the deflected thrust is required to act.

Now clearly the line of action of the deflected thrust must pass through, or close to, the centre of gravity of the aircraft, otherwise large changes of trim will occur when the jet is deflected. This implies that the deflection device itself must be close to the centre of gravity and for this reason alone nozzle deflector vanes or swivelling jet pipes are unlikely to be practical deflection devices. Even if the engines were placed in an acceptable position, as could be done in a new design, it seems likely that this would result in the performance of the aircraft being undesirably compromised. The use of an alternative nozzle in conjunction with some form of diverter valve appears, therefore, to be the most practical arrangement.

2. General Design Features

Having stated the main reasons leading to the choice of a deflector located in the jet pipe, some general features influencing the detailed design of units of this type will now be considered.

Most important is the influence of the deflector on the engine operating conditions. In the two extreme positions the effect can be eliminated by suitably trimming the final nozzles, but it is difficult to avoid some interference when the deflector is passing through its intermediate positions. Fortunately the increase in effective nozzle area, which occurs with most deflector designs, acts in a "safe" sense and causes the engine operating line to move away from surge and correspondingly reduces the turbine inlet temperature.

Another important consideration is the loading likely to be incurred during actuation. During preliminary discussion of the flight programme for the Meteor tests it was agreed that the engines would be throttled back to idling speed during the approach and the jets deflected at this condition. Once deflected the thrust would be increased to the value required for landing. Although this operational technique was expected to be adopted it was decided that for the purposes of determining the actuator size and for stressing the deflectors and their operating linkage, the case of operation at half thrust condition would be taken as standard. In the event this proved to be the normal operating condition.

3. Thrust Requirements for Meteor Deflector

Turning now to the particular experiment with the Meteor, preliminary calculations showed that for the approach speed to be significantly reduced the (deflected thrust/weight) ratio had to be at least 0·4 and preferably in excess of 0·6. To meet this requirement and to ensure that the deflected thrust line was correctly located, it was necessary to modify the aircraft by moving the power plant forward and replacing the normal Derwent engines by Nenes. The aircraft was balanced by removing the armament, armour and ballast from the nose and adding some ballast to the tail. To improve the lateral control and to cater for an engine failure with the jets deflected, wings of larger span were fitted.

The performance required from the thrust deflector was governed primarily by the baulked landing case for it was considered necessary for the aircraft to be capable of climbing at an angle of 5° with jets deflected. This requirement originally led to the stipulation that the

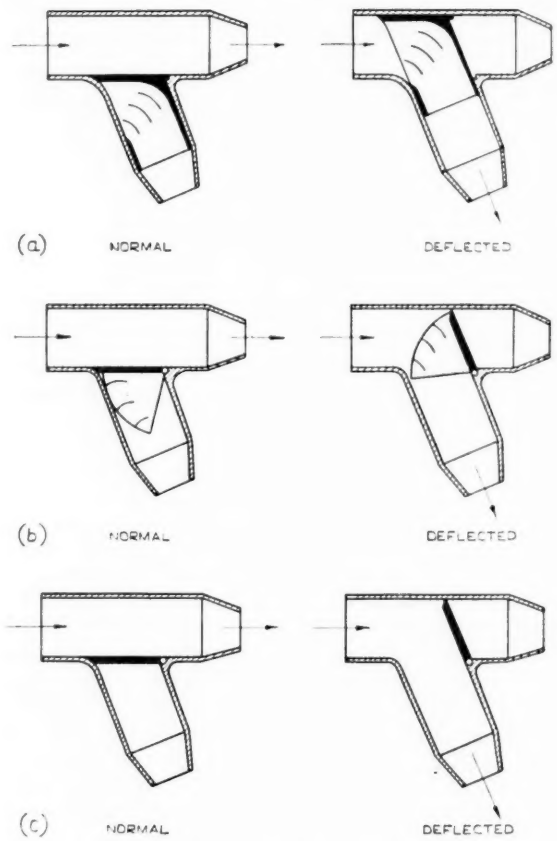


FIGURE 1. Model deflector units based on square section ducts.

deflected thrust from each engine had to be at least 4,450 lb. and the deflection angle 70°, but soon after development of the deflector had started and, as the result of more detailed aircraft performance calculations, the deflection angle was reduced to 60°, although the thrust requirement remained unchanged.

The development of the deflector proceeded in three stages: an examination of possible schemes and an evaluation of the aerodynamic performance of the most promising on model scale, detailed mechanical design of the full size deflector, and engine development leading to flight clearance tests. Each of these phases will now be described in detail.

4. Examination of Possible Schemes

Four deflector schemes were prepared; the main differences between them concerned the shape of the jet pipe cross section and the manner of deflecting the hot gas stream and sealing the valves within the casing. All four schemes were tested in model form but, largely as a result of weight estimates made for the full scale versions, three were abandoned after preliminary tests had been made and only the fourth model became the subject of detailed testing.

The first three schemes (Fig. 1) were based on the use of square section ducts for the deflector unit and deflected jet pipe with transition pieces from square to circular section between the unit and the engine exhaust cone and the normal jet pipe. Square section ducts are attractive in that they give greater scope for the use of rotating and sliding deflection members, such as flap valves or sliding shutters and, to some extent, they ease the sealing problems involved. A square section also permits the use of full-width cascade vanes for turning the hot gas stream and it was therefore considered that the use of a square section would help to keep to a minimum the losses incurred in the deflection process. It was feared, however, that the weight might become excessive owing to the stiffening requirements of the flat duct walls and in fact this did prove to be the case.

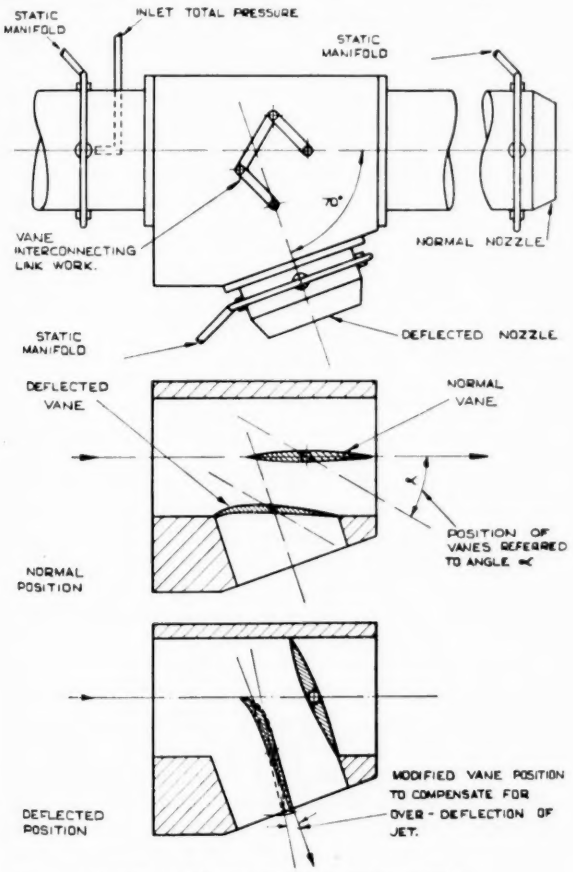


FIGURE 2. Diagram of model deflector unit.

All the three schemes shown in Fig. 1 were tested in model form and there appeared to be nothing to choose between the plain and cascaded flap valves (Figs. 1(*c*) and (*b*)) on the score of loss of thrust when running deflected. The cascade box (Fig. 1(*a*)) gave a slightly inferior performance and was abandoned at an early stage on the grounds of mechanical unsuitability.

The fourth scheme, based on circular section ducts, utilised two butterfly valves carried on transverse spindles and so positioned and coupled that each jet pipe in turn was sealed off, while the other remained open. It was this design that was chosen for detailed testing and full-scale development.

5. Test Model and Thrust Deflection Rig

All the aerodynamic tests were made on a model giving a nominal deflection angle of 70°. This model, which is shown on Fig. 2, was made to approximately quarter scale and correctly simulated the internal flow areas, reproducing as closely as possible the contours of the vanes. The bearings supporting the vane spindles could not be exactly reproduced and for simplicity it was decided to use steel spindles running in plain bronze bearings. Provision was made for attaching to either vane spindle a quadrant arm from which a scale pan could be suspended for the measurement of torque.

The layout of the model test rig is shown in Fig. 3. Spring balances were used to measure the horizontal and vertical components of the thrust and conventional methods were used to record the static air pressure immediately upstream of each of the two nozzles and the total pressure at entry to the unit.

Preliminary tests were made to see the effect of the air flow upon the coupled vanes and these showed that the vanes would only remain stable in the straight-through position at very small pressure ratios. At higher pressure ratios the vanes moved of their own accord to the deflected position and remained there, hard on the stops, but with no tendency to vibrate.

During these tests it was observed that when running at the higher end of the mass flow range the axis of the deflected jet made an angle of approximately 8° with the axis of the deflected jet pipe. A few simple tests showed that this "over-deflection" could be compensated for by adjusting the coupling linkage so that the deflected vane moved some 8° beyond its nominal fully-open position.

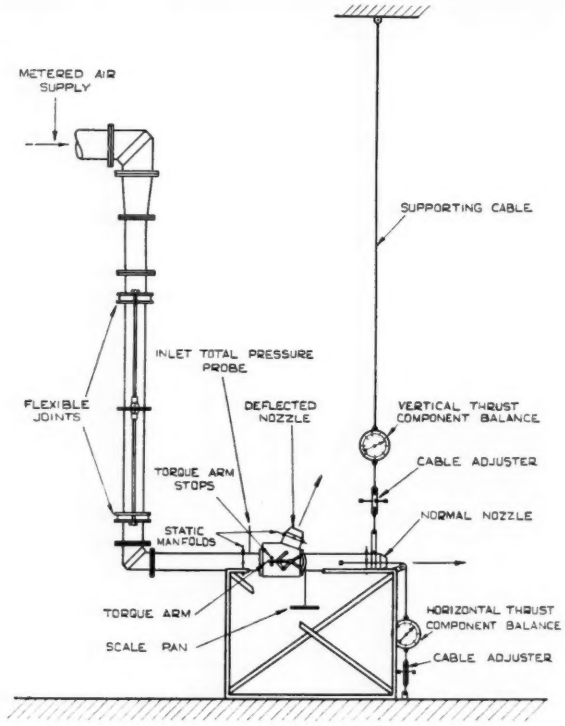


FIGURE 3. Layout of model test rig.

5.1. TORQUE MEASUREMENT

Figure 4 shows the variation with vane position of the torque due to the coupled vanes. The shape of the torque curve can be accounted for as follows: over the first 10° of movement the torques from each vane are additive, with the influence of the normal vane predominating. During the next 10° the torques of both vanes are reduced, until at approximately 20° the deflected vane reaches a "dead region" within which it shows no inclination to move in either direction. This is followed by a reversal of torque due to this vane and, together with the falling torque of the normal vane, this results in a downward trend which reaches a minimum at approximately 36°. From this point onwards until just before the fully deflected position is reached the torque due to the deflected vane rises rapidly and in its original sense to reach the maximum of the whole cycle.

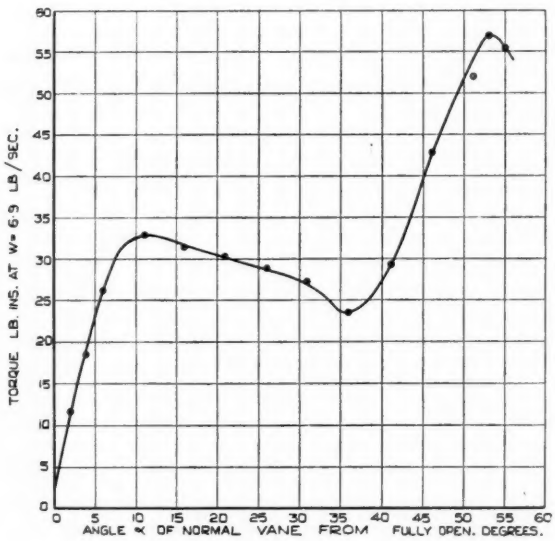


FIGURE 4. Variation of torque with vane position.

The prediction from the model tests of the torques to be expected in the full-size deflector unit was made difficult not only by the unusual geometric configuration of the vanes and flow paths but also by the nature and temperature conditions of the proposed full-size bearings which could not be reproduced in the scale model. The model bearing friction component was of course included in the measured gross torques and it was not possible to determine its magnitude independently. However, since it was considered that the major portion of the torque was due to the aerodynamic forces on the vanes, the measured gross torques were used to predict the full-scale values.

Figure 5 shows the predicted values of torque for the full-size unit for conditions of full thrust and half thrust at ground level. The values quoted are those exerted by the two coupled vanes upon the operating mechanism during the change from normal to deflected jet. Under these conditions the forces on the vanes supply the torque to overcome bearing friction, and the operating mechanism merely imposes a braking effect on the vanes to prevent them "slamming" on to the stops at the end of their travel. On the other hand, to return the jet to normal, the operating mechanism has to supply sufficient torque to overcome the sum of the separate torques due to aerodynamic loads, bearing friction and, initially, the inertia of the vanes themselves. An estimate was made of the torques attributable to the latter two effects and a final value of 600 lb. ft. arrived at for the maximum gross torque required from the mechanism during the operating cycle at half thrust, ground level conditions.

The model tests also indicated that in designing the full-size unit steps should be taken to ensure that in the closed position the vanes did not touch the casing so that any possibility of the deflector becoming jammed due to distortion of either the vane or the casing could be avoided. It was thought preferable to limit the vane travel by means of stops fitted to the casing rather than to allow the loads to be taken continuously by the operating mechanism. It was also considered desirable to fit a positive mechanical lock to secure the vanes in the normal position. Both of these features were incorporated in the final design.

5.2. THRUST MEASUREMENT

The thrusts from the normal and deflected jets are compared on a non-dimensional basis in Fig. 6, that for the deflected jet being calculated using the resultant of the measured horizontal and vertical thrust components. The curve of deflected thrust takes into account the enlarged deflected nozzle and the modified "fully-open" position of the deflected vane. At a

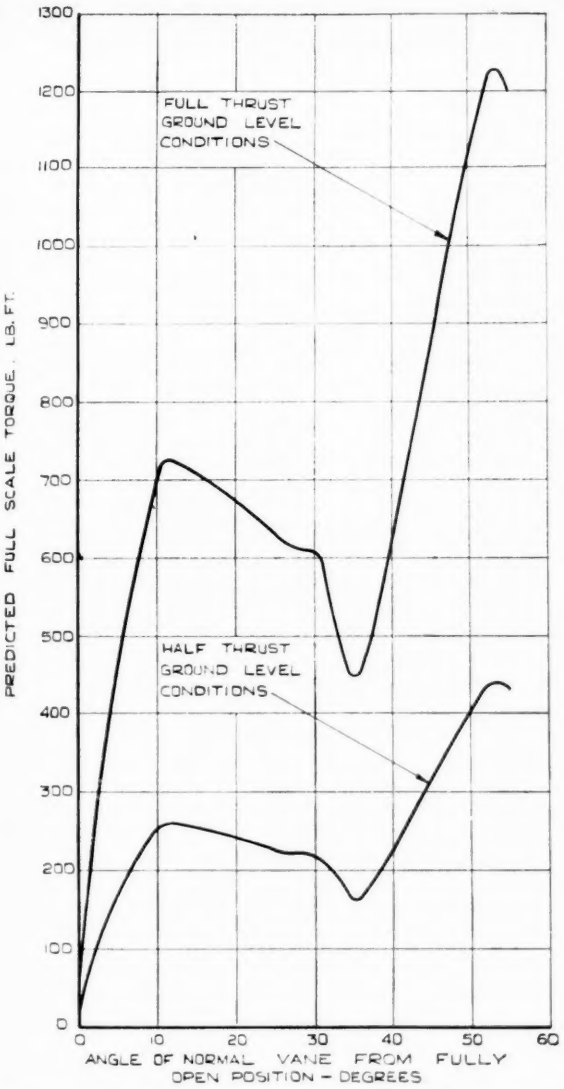


FIGURE 5. Variation of predicted full-scale torque with position of vanes.

pressure ratio corresponding to full thrust conditions the thrust loss due to deflection amounted to 12 per cent.

The angle of deflection calculated from the measured thrust components was found to vary by less than $\frac{1}{2}°$ from the nominal deflection angle of 70° in that part of the pressure ratio range over which the full-scale deflector unit would operate.

6. Mechanical Design of Full-Scale Deflector

Since the primary purpose of the experiment described in this paper was to obtain flight test data, the stress levels used in the deflector design were deliberately chosen to be conservative to ensure adequate reliability. Although this policy resulted in some additional weight penalty, it was proved to be fully justified by the behaviour of the deflectors during the flight trials for no serious mechanical defect occurred during that time.

6.1. General design features

Each deflector unit consisted simply of two pipes intersecting at 60°, the horizontal section constituting part of the normal jet pipe with the other pipe carrying the deflected jet and expelling the exhaust through an opening under the engine nacelle. To ensure synchronised operation of the two deflectors, they were operated by a single hydraulic actuator mounted in the fuselage.

The deflectors were rigidly attached to the airframe by means of struts fixed to the front and rear spar reinforcements of the nacelle; an additional strut prevented lateral movement. This rigid mounting system required flexibility in the connections between the engine, deflector and jet pipe and this was provided by a special double spherical joint attached to the engine exhaust cone and by a swinging link support for the main jet pipe.

The deflector vanes were supported on stubshafts supported on plain spherical bearings turning in housings welded to the deflector body. These bearings were sealed from the gases in the jet pipe by introducing cooling air from the engine compressor into the hollow stubshafts and passing this air through six small holes into an annulus turned in the shaft between the bearing and the inside of the jet pipe. A bearing cover fitted on the outside of the housing contained both the lubricating grease and the cooling air inside the bearing;

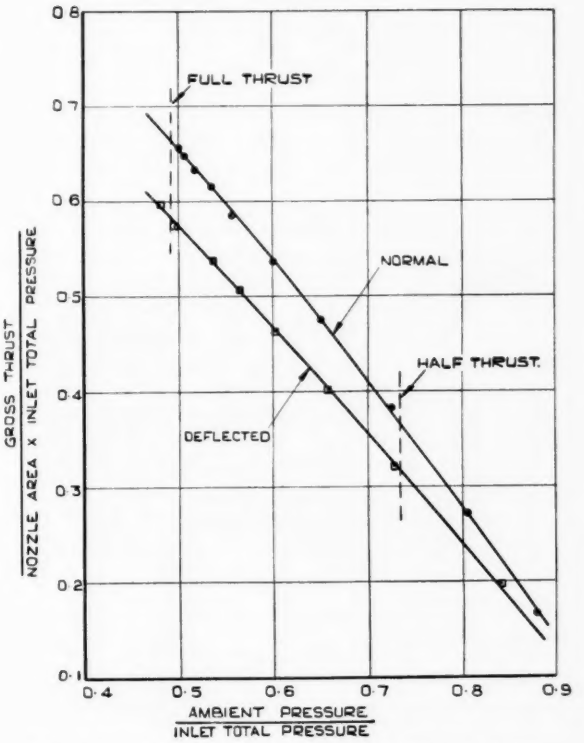


FIGURE 6. Variation of non-dimensional thrust with pressure ratio for normal and deflected jets.

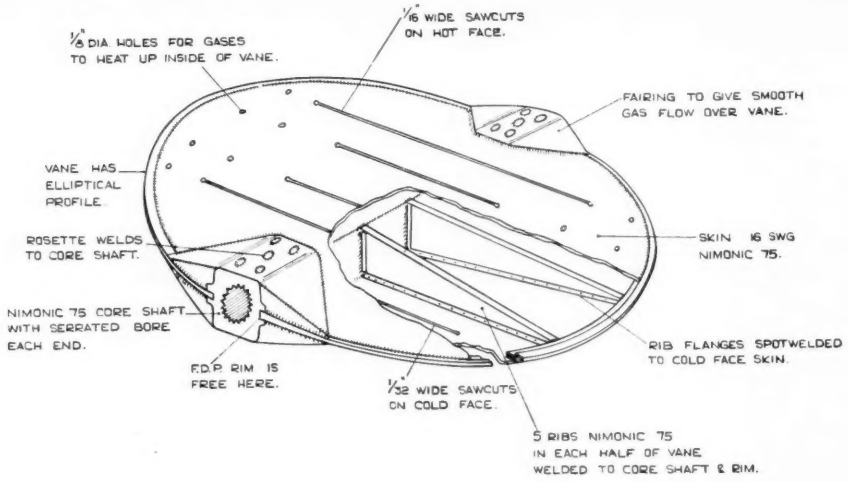

1/8" DIA HOLES FOR GASES TO HEAT UP INSIDE OF VANE.
1/16" WIDE SAWCUTS ON HOT FACE.
FAIRING TO GIVE SMOOTH GAS FLOW OVER VANE.
VANE HAS ELLIPTICAL PROFILE.
ROSETTE WELDS TO CORE SHAFT.
SKIN 16 SWG NIMONIC 75.
NIMONIC 75 CORE SHAFT WITH SERRATED BORE EACH END.
RIB FLANGES SPOTWELDED TO COLD FACE SKIN.
F.D.P. RIM IS FREE HERE.
1/32" WIDE SAWCUTS ON COLD FACE.
5 RIBS NIMONIC 75 IN EACH HALF OF VANE WELDED TO CORE SHAFT & RIM.

FIGURE 7. Typical section through a vane.

in addition it forced the cooling air back into the bearing and thus assisted thermal insulation.

To avoid the possibility of the vanes becoming jammed they were designed so that the minimum radial clearance between the inside of the deflector body and the vane rim was 0·025 in. A careful check of this clearance was kept both during the engine development tests and during the flight trials.

6.2. Vane construction

In general the construction of the deflector components followed established practice, but a description of the method used for the vanes may be of interest.

Figure 7 shows a typical section through a vane. The assembly comprised a central core of Nimonic 75 material bored and serrated at each end to take the stub-shafts which supported it in the deflector body. The vane rim was of FDP Steel (D.T.D. 571) approximately 1 in. × $\frac{3}{8}$ in. section and to this were welded two skins of 16 SWG Nimonic 75, the two skins being dished in the centre to allow for fitting and forming over the core shaft at the centre-line of the vane and at the rim edges. Fairings were welded on the minor axis of the vane at the outside edges to avoid sharp changes of section at the core shaft ends and to give a reasonable flow path to the gas passing over the surface. The skins were supported by five ribs welded to each side of the core tube and also to the vane rim; the skins were spot-welded to these ribs on the cold side only, as the gas pressure on the hot side pressed the skin against the rib flange.

A perspective drawing of the complete deflector unit is given in Fig. 8. This also shows the lever system for coupling and actuating the vanes as well as other detail features.

6.3. Bearing lubrication

Tests were made to determine the most suitable lubricant for use in the spherical bearings carrying the vane spindles. The temperature of these bearings was found to be in the region of 450°C and a grease was required which would remain in the bearing for the duration of at least one flight before replenishing.

Three types of lubricant were tested, namely: —

(*i*) Graphite grease.
(*ii*) Colloidal starch.
(*iii*) Molybdenum disulphide grease.

The first grease oxidised in the bearings, the second formed a carbon film and was partly successful, but the third was found most suitable and had the added advantage that it could be applied through the grease nipple provided.

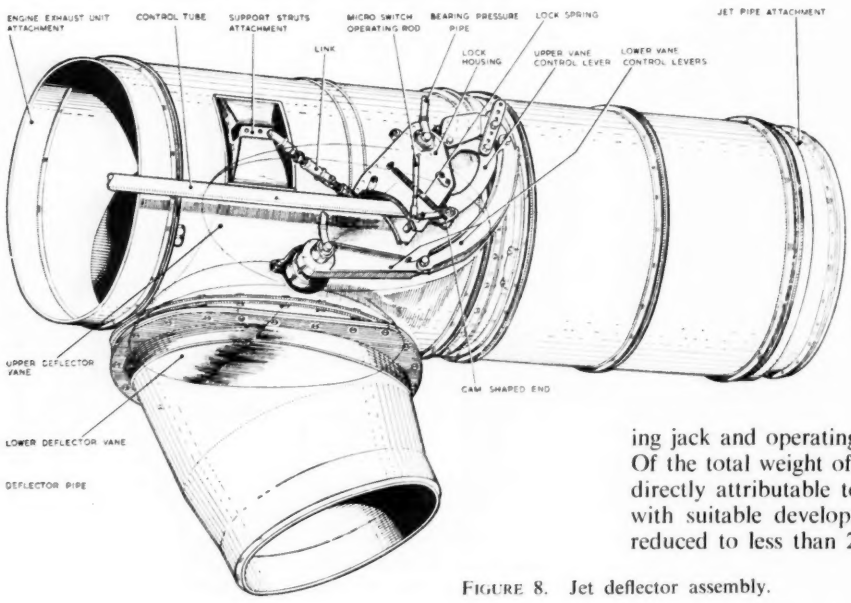
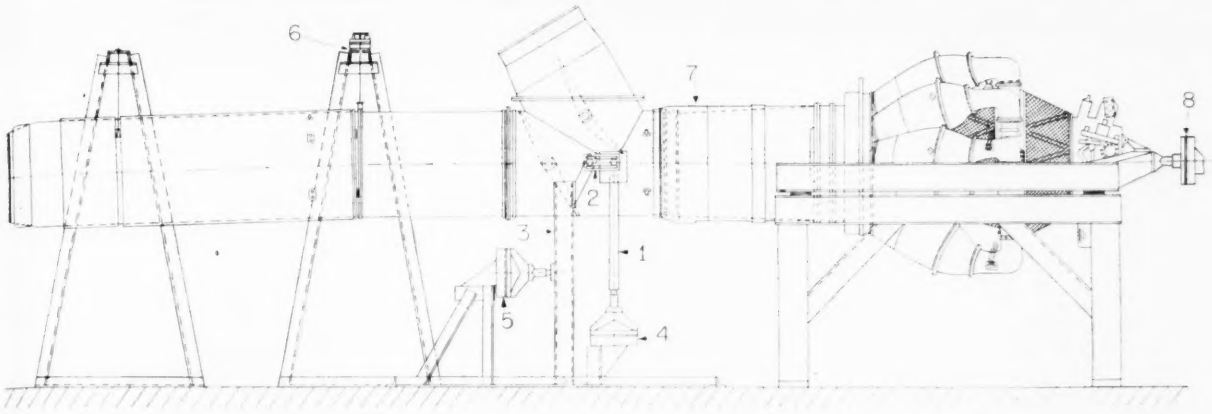


FIGURE 8. Jet deflector assembly.

6.4. Weight analysis

A weight breakdown for one complete deflector installation, exclusive of the actuating jack and operating linkage, is given in Appendix I. Of the total weight of 411 lb., approximately 292 lb. is directly attributable to the deflector; it is thought that with suitable development this latter weight could be reduced to less than 250 lb.



1. Strut carrying main vertical force on deflector.
2. Link to transfer horizontal force on deflector to 3.
3. Pivoted box beam.
4. Precimeter measuring main vertical force.
5. Precimeter measuring horizontal force.
6. Precimeter measuring residual vertical force.
7. Sliding joint between engine and deflector.
8. Precimeter measuring engine bearer force.

FIGURE 9. Layout of thrust rig.

7. Engine Tests of Deflector

This section of the paper deals with the mechanical testing and measurement of the performance of the full-scale deflector. These tests involved first of all some detailed development work on two prototype deflectors and then, when a satisfactory performance had been achieved, a 25-hour flight acceptance test. Subsequently 5-hour acceptance tests were made on each of the two flight deflectors before they were installed in the airframe.

Although the bench testing began on a unit having a deflection angle of 70°, and in fact a total of $74\frac{3}{4}$ hours running time was accumulated with this, except where specifically stated all the comments which follow refer to tests on the 60° prototype unit.

7.1. Details of thrust stand

A test bed was required which would carry a Nene engine and on which the deflector could be mounted with clear space for a jet deflected at 60° to the horizontal. These requirements could be most easily met on an outside test bed and such a bed was chosen for the tests. The deflector was tested upside down so that the deflected jet pointed upwards into the air. The layout is shown diagrammatically in Fig. 9.

The engine was mounted on a cradle free to move horizontally on rollers, its forward movement being restrained by a thrust measuring device (N.G.T.E. precimeter), which is shown as Item 8 in Fig. 9. The deflector was carried on a pair of trunnions which were positioned to lie at the intersection of the straight and deflected jets. These trunnions were supported by a vertical strut (1), which transmitted the load directly to precimeter (4), and a horizontal link (2) which transferred the horizontal force to the bracket and pivoted box beam arrangement (3) and thence to precimeter (5). The box beam formed a 2:1 lever system so that the force measured by precimeter (5) was twice the horizontal force on the deflector. In addition to the forces acting on the trunnions there was a moment due to the resultant force not acting through the point of support. This was measured by the small precimeter (6) attached to the rear of the deflector.

In order to avoid errors in force measurement it was necessary to ensure that the coupling between the engine and deflector allowed complete freedom of axial movement. Frequent checks were made during the test programme to see that this condition was achieved.

7.2. Preliminary mechanical tests on 70° deflector

The initial running on the 70° deflector was done to check the complete assembly and installation mechanically. During these runs it was found that the vane skins had buckled, but this trouble was successfully cured by milling four longitudinal slots on each face to relieve the skin stress and by blanking off the cooling air passage leading to the centre core of the vane spindles to reduce the thermal gradients.

In subsequent running serious cracking occurred at the base of the leading edge of the deflected nozzle and this was overcome by changing to a stiffer double-skinned design. Both of these design modifications were incorporated into the 60° flight deflectors.

Measurements were made of the operating torques and it was found that at half thrust conditions the torque required to return the jet to normal was 560 lb. ft., a value which corresponded reasonably well with the value predicted from the model tests. The engine tests also confirmed the tendency of the vanes to move into the deflected position and emphasised the importance of providing an adequate locking mechanism to hold the vanes securely when running in the straight-through position.

7.3. Tests on prototype 60° deflector

Most of the tests on this unit were made at conditions varying from maximum continuous cruising upwards to establish the relationship between the engine speed and the jet pipe temperature and thrust and to examine the effects on these quantities of small changes in the internal geometry. Before these tests began it was clear, both

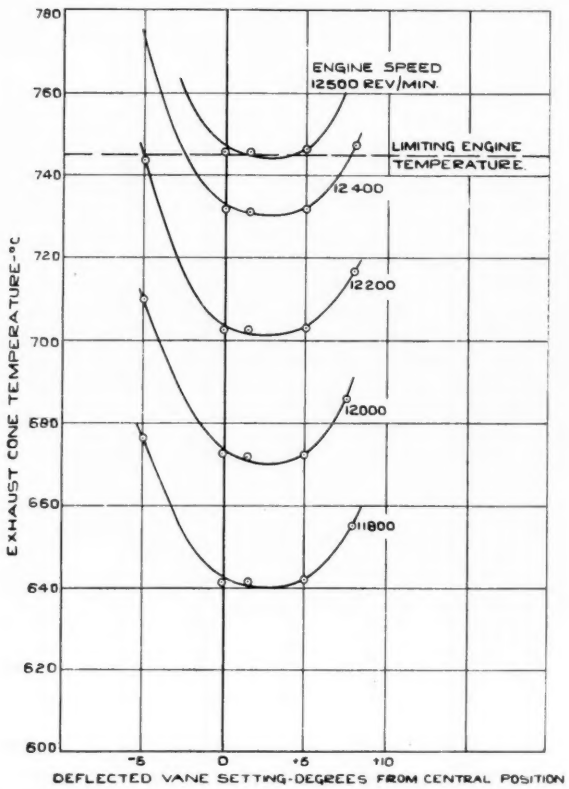


FIGURE 10. Variation of exhaust cone temperature at constant engine speed with setting of deflected vane.

from the model tests and from the full-scale tests on the 70° deflector, that the required deflected thrust would not be achieved so long as the engine was restricted within its normal temperature limitations and so it was agreed that the engine could be run for a limited period at a temperature 25°C in excess of the normal take-off value.

Once the size of deflected nozzle had been determined, tests were made to find the effect of changing the deflected vane setting to see if any variation in deflection angle was obtainable. It was found that while some variation could be achieved, the gas temperature rose sharply when the vane was moved outside the range 0° to 5° over-deflected and this prohibited settings in this region being used. This is shown in Fig. 10. However, the tests showed that the highest thrust was obtained with the vane 5° over-deflected and this position was therefore adopted as standard.

A further test was made to measure the operating torque and it was established that at half thrust conditions the torque was 750 lb. ft. and the times to deflect and return the jet to normal were approximately 0·8 and 1·0 seconds respectively.

7.4. Flight acceptance test

At the conclusion of the development tests a 25-hour test, made to the schedule set out in Appendix II, was carried through without incident. Close watch was kept for cracks in the skin of the vanes and although cracks were observed which slowly increased in length during the test, they were not considered to be dangerous.

8. Performance Measurements on Flight Deflectors

The two flight deflectors were made identical with the developed prototype and so a limited test schedule of two hours duration was considered adequate to prove them. Before this test was done the sizes of the deflected nozzles were adjusted to obtain the desired gas temperature; the performance was then measured over a range of engine speeds.

The measured performance in the deflected and straight-through positions is given in Fig. 11. At take-off conditions (12,500 r.p.m.) the straight and deflected thrusts were 4,865 lb. and 4,730 lb. respectively, the corresponding gas temperatures at the deflector entry being 715°C and 733°C. Thus while the thrust loss due to deflection amounted to only 2·8 per cent, this low value was obtained by allowing the gas temperature to exceed the normally permitted value.

If the performance is related to that of a standard engine and no temperature increase is allowed, fitting the deflector reduces the straight-through thrust by 5 per cent, of which loss perhaps half is due to gas leakage past the deflected vane. The loss when deflected is $10\frac{1}{2}$ per cent and if $2\frac{1}{2}$ per cent of this is again assumed to be due to leakage, the resulting "aerodynamic" loss of 8 per cent compares reasonably well with the value predicted from the model tests after scaling by a factor of 0·7 to allow for the change in deflection angle from 70° to 60°. This scaling factor was obtained by examination of the results of tests on simple mitred bends.

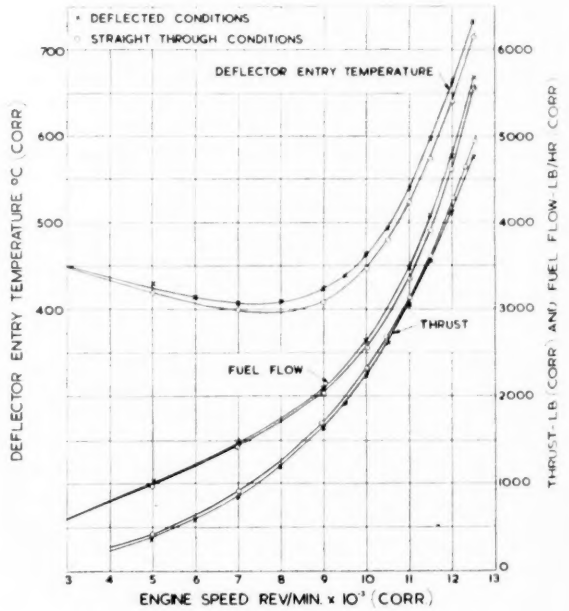


FIGURE 11. Complete performance calibration.

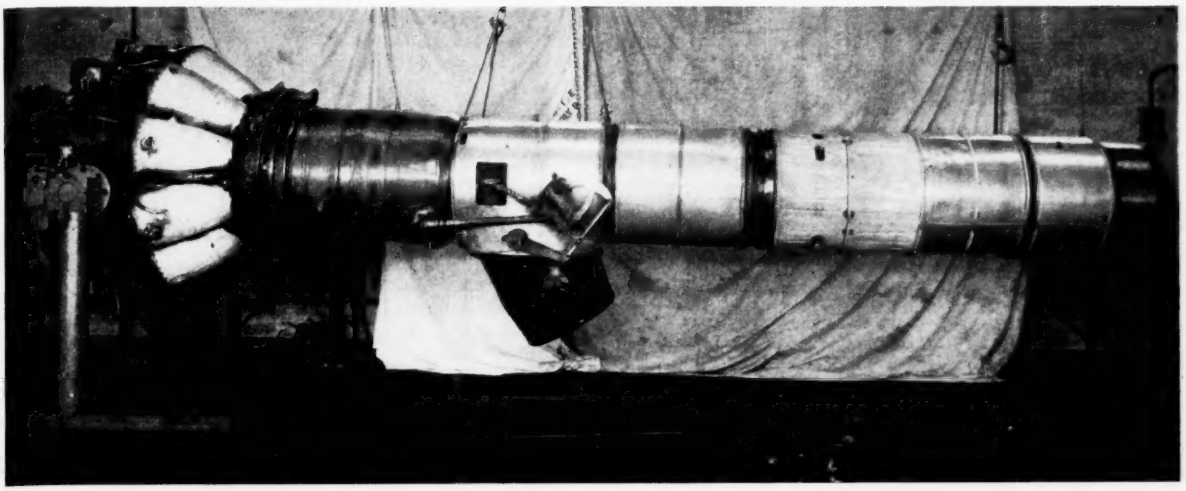


FIGURE 12. Engine, deflector and jet pipe assembly.

9. Conclusion

This part of the paper has described the development of the full-scale flight deflector to the successful completion of a 25-hour flight approval test. The required performance was achieved and the mechanical behaviour on the test bed found to be completely satisfactory.

After the first 25 hours of flying, both deflectors were removed from the aircraft and returned to N.G.T.E. for examination. Apart from small cracks in the vane skins extending from the ends of the sawcuts and some parting of the rosette welds to the core shaft, no mechanical deterioration was found and a further clearance was therefore given for another 25 hours operation. Two subsequent extensions of 25 hours were given as the result of inspections made on the deflectors *in situ*.

It can therefore fairly be claimed that the jet deflectors were successful in enabling the desired flight test programme to be carried through and that their performance was consistently satisfactory.

A photograph of a complete deflector installation is given in Fig. 12.

PART II—FLIGHT TESTS

10. Description of the Aircraft

The test aircraft—Meteor RA.490—started life as one of a small number of Mark III aircraft which had specially modified centre sections to adapt them as flying test beds for axial flow jet engines. At the extremities of the centre section main spars, the normal mounting rings for the Derwent engines had been replaced by heavily reinforced arches, allowing the test engines to be undersiung and increasing the span by about one foot.

For the purposes of the present experiment the existing front fuselage and centre section were retained. The arched spars accommodated the jet pipes of the Nene engines, which had to be mounted ahead of the front spar to allow the deflected thrust line to pass close to the centre of gravity. This resulted in the characteristic long nacelles which extended some 8 ft. ahead of the leading edge. They are well illustrated in the photographs of Fig. 13.

To balance the lateral component of the intake momentum drag on these long nacelles and, to a lesser extent the effect of their increased side area, some additional fin area was required. This was provided by small extra fins mounted on the tailplane of the Mark 8 rear fuselage, which replaced the original Mark III.

Lateral control in the event of an engine failure while deflected would clearly be a critical condition. The maximum possible amount of control was provided by fitting standard Meteor P.R. Mk. 10 outer wing panels. These increased the span to 44 ft. 4 in.—a record for Meteors.

The nose and main undercarriage units were, respectively, those of the N.F. 11 and the Meteor F. 4.

A three-view general arrangement drawing of the aircraft is shown in Fig. 14.

The engines were mounted in box-section rings supported at the forward ends of beams which extended forward from brackets in the front spar arch. Shortened exhaust cones connected the engines via flexible joints to the deflector units which were mounted between the main spars. These deflectors were rigidly attached to the front spar rings and to the nacelle structure, so that the engine mountings carried none of the deflected thrust. The straight-through jet pipe extended aft from the deflector unit through the rear spar arch to the rear of the nacelle.

The deflected jet pipes were very short and extended only just clear of the undersides of the nacelles. Even so, with the tyres and oleos fully compressed, the forward edges of the pipes were only an inch or so clear of the ground. This is clear from Fig. 15, which also illustrates the large overhang of the engine mounting.

The pilot's control of the deflector mechanism

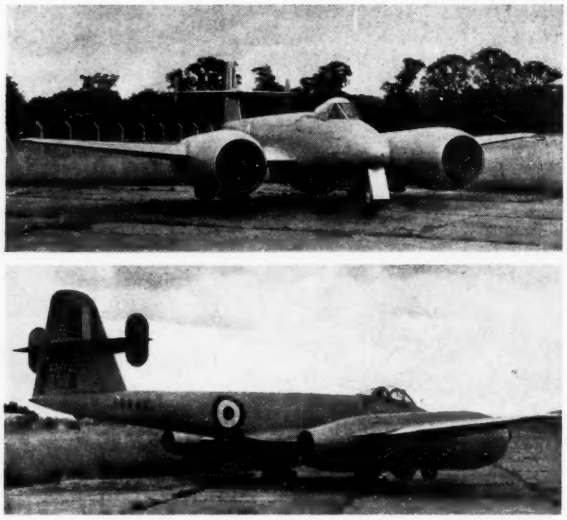


FIGURE 13. Front and rear views of the test aircraft.

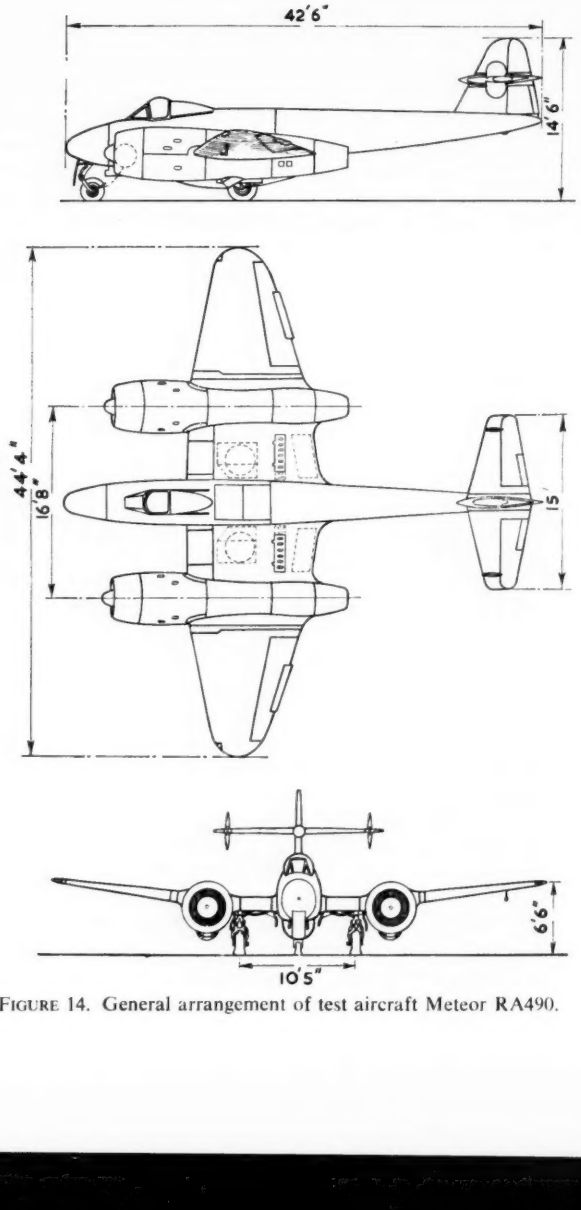


FIGURE 14. General arrangement of test aircraft Meteor RA490.

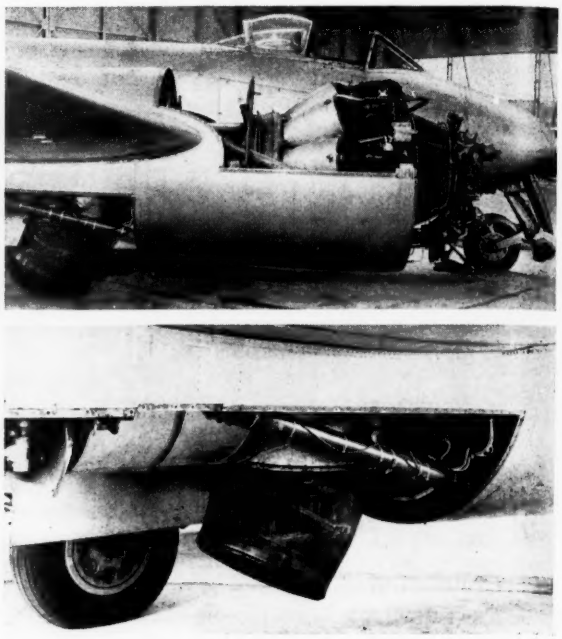


FIGURE 15. Views of engine mounting and deflected jet-pipe.

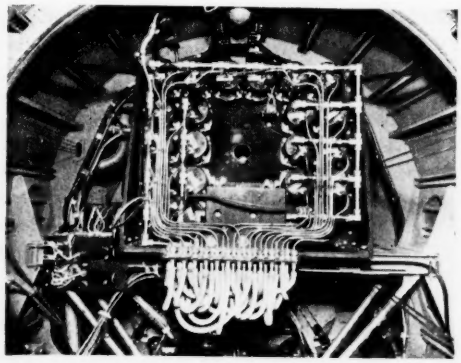
consisted of a small lever, moving vertically, on the port side of the cockpit above the throttles. Because of the limitations to the allowable torque on the deflector operating mechanism, it was necessary to restrict the operation of the deflector to engine speeds below 10,500 r.p.m., corresponding to one half static thrust at sea level. This was achieved simply by a cut-out switch on the throttle quadrant. However, since the deflection operation took only one second, it was possible to close the throttles to the "arming" position, operate the deflector, and open up again before much change in thrust occurred. This practice was discouraged of course, except in emergency, but no harm to the mechanism was ever caused by it.

11. Instrumentation

Normal instrumentation requirements were considered fairly early in the process of modifying the aircraft and the fixed equipment was built in by the design firm before delivery to the R.A.E.

Wing incidence had to be obtained by the laborious process of measuring aircraft attitude and flight path angle in steady conditions. At a later stage in the experiment, an instrument was fitted which indicated to the pilot how well a chosen incidence was being maintained and this proved to be of considerable assistance in controlling the aircraft. This equipment was not part of the normal instrumentation and will be described in more detail later (*see* Section 14).

Measurement of the deflected thrust in flight was a major part of the programme. It was thought originally that this could be done by indirect methods, using the results of the partial glide tests, but early analysis of the data cast doubts on the validity of the assumptions



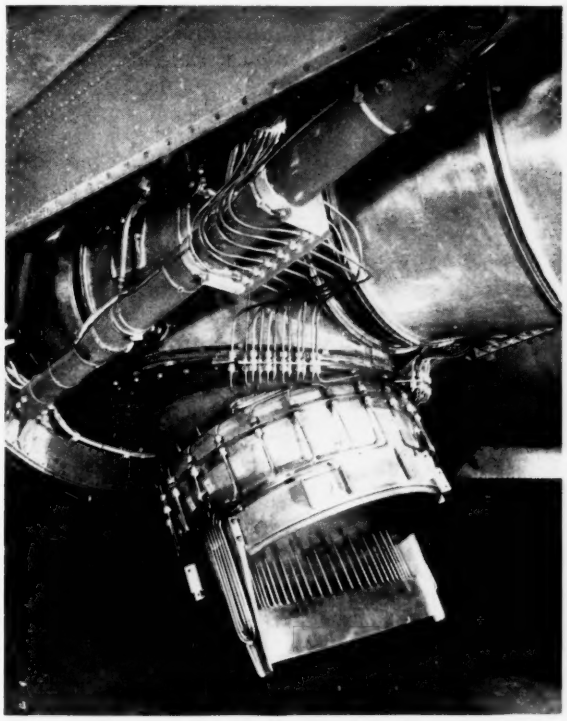


FIGURE 16 (*above and right*). Installation of thrust-measuring rake and recorder.

that had to be made, as discussed in Section 15.2. To resolve these doubts, it became clear that the thrust would have to be measured directly. For this purpose a rake was constructed consisting of 12 Pitot and 12 static tubes mounted on a streamlined strut. These 24 tubes were connected to 24 differential pressure gauges in a second auto-observer installed towards the rear end of the fuselage. The rake was based on an N.G.T.E. design and spanned the nozzle of the port deflected jet-pipe. It could be mounted across any one of four diameters, disposed symmetrically, with the tube orifices in the plane of the nozzle.

Figure 16 illustrates the method of attachment of the rake and shows the associated auto-observer equipment.

12. The Flight Test Programme

When the aircraft arrived at R.A.E. in August 1954, enough flying had been done by the design firm to show that the handling qualities of the aircraft were generally acceptable and that the deflection mechanism could be operated satisfactorily in flight.

The R.A.E. programme was broadly divisible into two parts, respectively qualitative and quantitative in nature. The two were considered to be of equal importance, particularly in the evaluation of some new concept such as this. The overall performances of the system could not be represented entirely in mathematical form, since it involved the pilot in a vital rôle and his impressions of the unconventional control problems raised were of great value.

12.1. Qualitative tests

These involved, firstly, a general assessment of the handling of the aircraft at low speeds with jets straight and deflected. Particular attention was paid to the effect on trim of operating the deflector mechanism and on the effectiveness of the controls at low speeds. This phase proceeded until the aircraft was being flown at full deflected power, at speeds some 25 to 30 knots below the normal (power-off) stalling speed.

Before approaches and landings could be attempted with jets deflected it was vital to establish the envelope of conditions within which this exercise was reasonably safe. Here again a qualitative assessment was adequate at this stage, although more precise information (from performance measurements) was available later.

The points on which the pilots needed to be briefed were (*a*) the combinations of height and air speed with jets deflected from which an over-shoot could be made without risk and (*b*) the minimum control speed in the event of one engine failure with jets deflected. These tests were, of course, made at a safe altitude, the results being extrapolated to ground level. They showed that the estimated climb performance was somewhat optimistic and resulted in the establishment of a "forbidden zone," for approaches with jets deflected, between 1,000 and 200 ft.

With these limitations tests were made to study the problems of flight path and air speed control at very low approach speeds. The severity of both these problems depends very largely on the accuracy with which a prescribed flight path can be defined for the pilot. In the absence of either airborne or ground aids for the purpose, the pilots generally aimed to fly an approach path terminating at a mark on the runway. For some tests a ground observer was provided with a theodolite set up to define a three degree approach path, information being passed to the pilot over the R/T, similar to G.C.A. procedure. Neither of these techniques was wholly satisfactory, but even this degree of control was sufficient to underline the sort of problems to be expected if tighter control of the path were to be used.

The pilots' reports were, in many cases, supplemented by auto-observer records of the type shown in

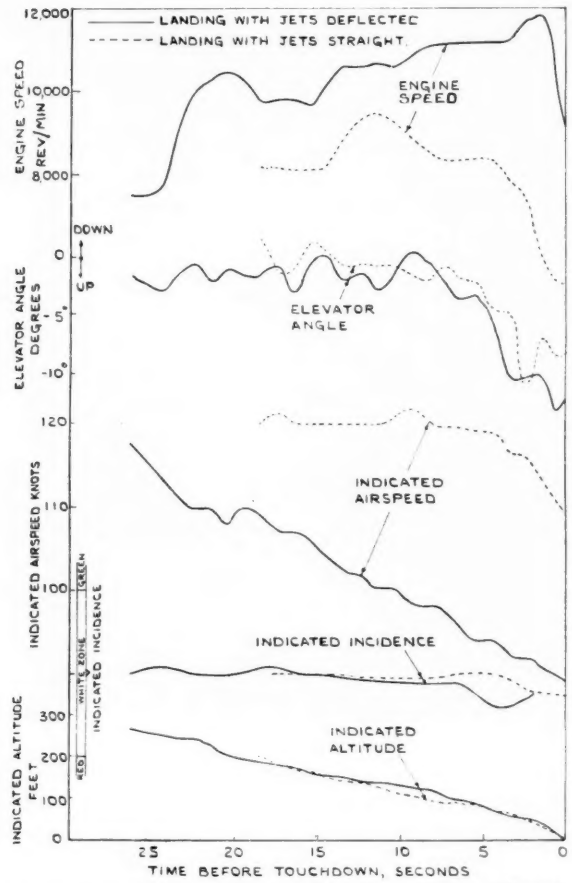
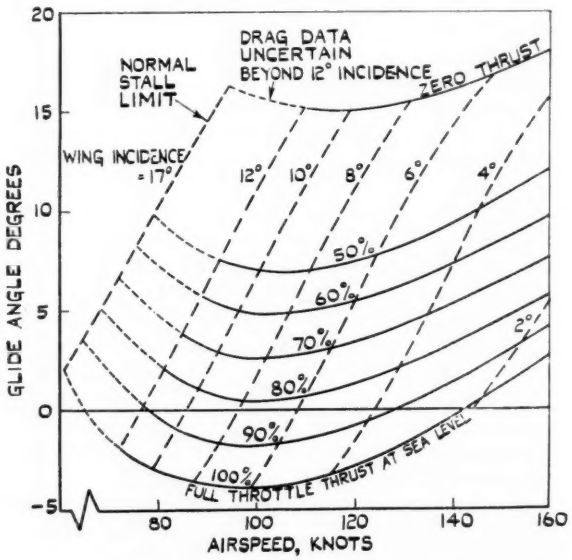


FIGURE 17. Typical time histories of landings with jets deflected and straight.



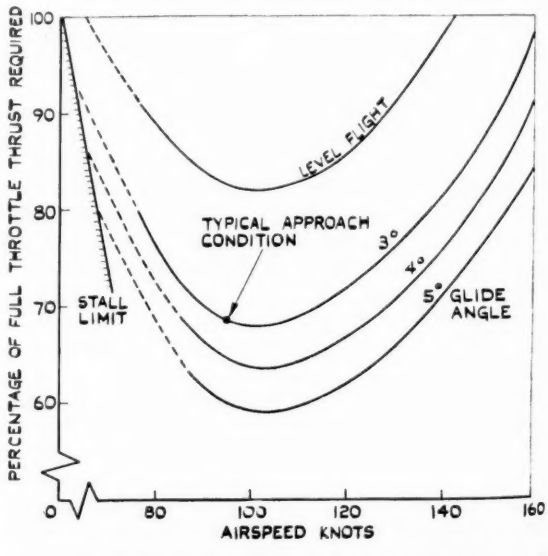
CURVES DERIVED FROM FLIGHT-MEASURED LIFT AND DRAG COEFFICIENTS, FLAPS AND UNDERCARRIAGE DOWN. WEIGHT = 15,800 LB. AT STANDARD SEA-LEVEL.

FIGURE 18. Performance with jets deflected.

Fig. 17 and by ground-camera records of the actual flight paths followed. This type of information is of use mainly in interpreting the pilot's remarks, or of adding precision to his record of speeds, engine powers, and so on. By themselves they are no substitute for a carefully written pilot's report. Long experience in attempting to analyse this type of record confirms the obvious fact that such records merely show what the pilot did and, much less clearly, what the effect of such action was. They seldom show why the pilot took such action, or what he was attempting to achieve by it.

12.2. Quantitative tests

These were concerned very largely with the measurement, in flight, of the forces produced by the deflected jets. The basic technique consisted of flying stabilised partial glides or climbs at a range of air speeds and engine powers, with jets straight and deflected. This is fundamentally the most accurate method of determining aerodynamic lift and drag, since measurements are made under equilibrium conditions. The obtainable accuracy is, however, critically dependent on atmospheric conditions and on the accuracy with which the pilot can maintain constant conditions throughout the glide or climb. The maintenance of a steady air speed was somewhat more difficult than on a conventional aircraft. At very low speeds, with jets deflected, response to controls was sluggish and, with a large proportion of the weight supported by engine thrust, the magnitude of the changes in aerodynamic lift and drag forces which could be produced by changes in incidence were relatively smaller than for an aircraft in which the wing lift roughly equalled the weight. If much turbulence was present, therefore, it was almost impossible to hold a steady air speed. Since it was not



CURVES DERIVED FROM FLIGHT-MEASURED LIFT AND DRAG COEFFICIENTS, FLAPS AND UNDERCARRIAGE DOWN WEIGHT = 15,800 LB, AT STANDARD SEA-LEVEL.

FIGURE 19. Thrust required for various glide paths with jets deflected.

practicable to wait always for flat calm conditions, we had to accept rather more scatter than usual in the derived results.

Analysis of flight test data demanded a knowledge of the position error of the air speed system, which consisted of a standard Pitot-static head on a boom attached to the port wing tip. A suspended static head was not fitted, since it would almost certainly have been seriously affected by the deflected jet efflux. Methods involving flying at very low heights and low speeds with jets deflected were considered dangerous, so that, for example, the aneroid method could not be used. It was found that a sufficiently wide range of air speeds and engine powers could be covered by the pacer method, using an Anson fitted with a venturi Pitot head and a suspended static for the low speed end of the range and another, previously calibrated, Meteor for higher speeds. This method involved accurate formation flying by very dissimilar aircraft, but quite good results were achieved by it.

A considerable amount of flying was devoted to the measurement of the deflected thrust using the jet pipe Pitot-static rake. From the pilot's point of view this was quite straightforward, requiring only a constant air speed and engine speed while a record was taken on the auto-observer. It was, nevertheless, a lengthy procedure because the rake had to be fitted in each of four positions, which meant a complete breakdown and re-assembly of the piping from the jet pipe to the pressure gauges at each stage (*see* Fig. 16).

13. Results of Qualitative Handling Tests

13.1. Handling at low air speeds

When flown as a conventional aircraft, *i.e.* with jets undeflected, the low speed and stalling behaviour was similar to that of a normal Meteor. In the landing configuration, *i.e.* with undercarriage and flaps down, at about 15,800 lb. weight (125 gallons of fuel remaining) mild buffet started at 120 knots and increased slightly as speed was reduced. At 98 knots a slight pitching oscillation started and mild dropping of either wing occurred. The nose dropped gently, with the stick almost fully back. Use of aileron reduced the wing drop to some extent.

These characteristics were the same with power either off or on (10,000 r.p.m.) except that in the latter case speed could be reduced to 94 knots, when more marked wing-dropping occurred. The minimum speeds correspond to lift coefficients of approximately 1·35 and 1·45 respectively.

The minimum comfortable air speed for the end of a power on approach was considered to be 110 knots.

With jets deflected, the (lower) speeds that could be achieved obviously depended on the amount of thrust that was being deflected. At a given engine speed, increase in altitude and/or ambient temperature caused a noticeable deterioration in performance.

The lowest indicated air speed ever reached was 65 knots with full power, although only one pilot ever achieved this. A more consistently achieved figure, at 2,000 ft. in normal winter conditions, was 75 knots. A general airframe buffet started at 105 knots, remaining moderately intense down to the minimum speed. The aircraft showed an increasing tendency to roll and yaw to port below 100 knots, the effect becoming quite strong below 80 knots. One third right rudder trim was required at 85 to 90 knots. At the minimum speed a gentle pitching oscillation occurred, with the stick two thirds of the way back from central. Slight forward movement of the stick would stop the oscillation, while further aft movement caused the port wing to drop. This minimum speed (75 knots indicated) corresponded to a gross lift coefficient of 2·3, showing that over 40 per cent of the weight of the aircraft was being supported by the jet thrust.

The rates of descent occurring during these speed reductions varied considerably with ambient conditions and with weight. On a hot day at 1,000 ft., pilots reported that height could be held only at speeds between 100 and 110 knots, in the landing configuration. On a cold day this speed band increased to about 90 to 120 knots.

The performance characteristics are discussed in more detail later, but reference may be made here to Figs. 18, 19 and 20 which summarise the results. Fig. 20, in particular, shows the effect of weight on performance at a given engine speed, and Fig. 19 shows the thrust required for a given glide path as speeds varied.

Control about the three axes at these low speeds was not seriously criticised by the pilots. Aileron and rudder responses were quite adequate, although the displacements were larger than usual and aileron control tended to become sluggish. Elevator response was less satisfactory and the situation was aggravated by the buffeting felt on the stick. Apart from the change in lateral/directional trim there were no specific comments on stability at these subnormal speeds.

Investigation of the minimum speeds to be used for the subsequent landing tests showed that, for the pilot, the first real limitation to reduction in speed with jets deflected was the rapid build-up in rate of sink, or loss in air speed following a disturbance. This is the familiar story of the problem of flight below the minimum drag speed. The partial glide tests, which are summarised

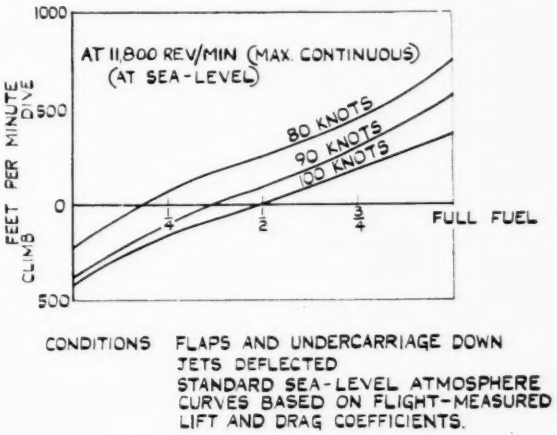


FIGURE 20. Effect of weight on performance.

in Fig. 18, show that at a typical engine thrust the speed for minimum glide angle (roughly equivalent to the minimum drag speed on a conventional aircraft) was about 100 knots. Below this speed the change in thrust per unit change in air speed became rapidly de-stabilising and, further, the thrust levels required for typical glide angles were much higher than those for a conventional aircraft. The pilot was therefore faced with an ever more difficult speed control problem as he reduced his air speed and he was also aware that the thrust he required was rapidly approaching the full-throttle value.

Pilot opinions naturally varied as to the seriousness of these problems at a given air speed and *minimum* approach speeds in the range 85 to 95 knots were quoted. There was, however, fair agreement that 95 knots with jets deflected was a fairly comfortable approach speed, in calm conditions.

Several pilots expressed the opinion that because the stalling speed varied with engine thrust, there was a danger of stalling the aircraft if thrust were reduced while flying at speeds below the normal (power-off) stalling speed. This fear presumably arose from the ingrained habit of associating the stall with a particular *speed* rather than with a particular incidence. In fact, tests showed that, even when flying at full throttle with the jets deflected, a sudden closing of the throttles would not cause a stall. The nose dropped rapidly as the flight path curved over following the large loss in lift and considerable height was lost before normal flying speed was regained, but no pre-stall phenomena were reported. It must be admitted that this behaviour was so similar to that following a straightforward stall that the difference was largely academic as far as the pilot was concerned, but the fact remains that no true stall was produced. The demonstration might have been more convincing had the Meteor exhibited a more vicious behaviour in the true stall—a violent wing-drop for example—which would not have been produced simply by closing the throttles.

13.2. Approach and landing tests

These tests were complicated by the safety limitation that flying with jets deflected should be restricted to a minimum for heights between 1,000 and 200 ft. Some approaches and landings were made with jets deflected all the way from 1,000 ft., but it was appreciated that the aircraft was committed to land from that point. Most of the work was done in two parts—(*a*) an approach control study, flown from 2,000 to 1,000 ft. and then discontinued and (*b*) a flare and touch-down study, in which the jets were deflected at 200 ft. after a conventional approach.

Pilot opinions of the approach problem in the 2,000 to 1,000 ft. height band reflect to some extent the degree to which they were trying to hold a prescribed glide path. No accurate means of defining such a path was available to the pilot and in many cases he simply flew an "estimated" three-degree approach path, or perhaps aimed to hold a constant rate of descent on his instruments. Hence, conditions were somewhat more favourable than they would have been operationally (using I.L.S., for example), and it is debatable as to how much of the pilots' difficulty was due to so-called minimum drag speed phenomena and how much to the simple fact that adjustment of engine thrust produced a change in lift which was several times *greater* than the change in longitudinal accelerating force, instead of being a negligible fraction of it. Use of the throttle to control the speed was therefore not only much less effective than normally, but it also tended to complicate the control problem by introducing probably unwanted changes in flight path angle as well.

Dealing with the speed control problem first, although in calm conditions a speed of 95 knots with jets deflected was considered adequate, any appreciable turbulence necessitated an increase, perhaps to 105 knots. The "effective-drag" data of Fig. 19 show the required changes in *total* thrust per unit change in air speed at these approach speeds. These changes indicate a speed instability at 95 knots which is very close to that which has been proposed as a limit for carrier-type approaches[1]. This strongly suggests that, on an aircraft with this type of jet deflection, speed stability should be assessed in terms of the changes in *total* thrust per unit change in air speed, rather than in terms of the changes in the thrust component along the flight path. This conclusion may not be valid, however, for an aircraft in which the two components of the thrust can be varied independently, *i.e.* if separate lifting and propelling engines are fitted.

The pilots tended to use the elevator to facilitate changes in air speed, leaving flight path control mainly to the throttle. The functions of the two controls could not be very clearly divided in this way, of course, since each control influenced both air speed and glide path.

One pilot found some assistance in achieving a desired speed by varying the flap angle from one third to fully down, with the thrust held fixed. In his view a fully variable drag control could replace the throttle control on this type of aircraft. With the elevator used to determine the speed, control would then have been close to that of a conventional aircraft. In other words, his plea was for a direct control of the longitudinal thrust-drag balance, with no effect on lift.

Glide path control via the throttle was fairly effective. The control was powerful, and always in the correct sense. Gaining height was particularly easy, provided that the speed had not got so low that the full-throttle limit was being approached. Losing height, or steepening the glide path, was, if anything, too easy. More than one pilot was unpleasantly surprised by the effect of a reduction in power. The consequent rapid increase in rate of descent required a very large application of power to arrest it and it was generally agreed that this was the most difficult correction manoeuvre of all. If, instead, the elevator were used to steepen the glide path the speed would increase as well and these changes took a long time to die out. If this height correction was made late in the approach, large variations in touch-down speed were unavoidable.

Much of the assessment of the handling on the final stages of the approach had to be done by delaying

deflection of the jets until the height was around 200 ft. The technique was to establish the desired flight path with jets straight at the normal approach speed of 120 knots. At 200 ft., the jets were deflected and power reduced while the original glide path was held constant, so that the speed began to fall. As the desired approach speed was approached, power was applied to achieve equilibrium conditions. A typical landing of this type is illustrated in Fig. 17. The process took time and left little in hand before touch down, so that examination of the control problem in this zone was difficult. Response to stick and throttle was much the same as at altitude, but here the pilot was much more aware of any deviation from the desired path and he was thus more critical of the controls.

The pilot's aim in this exercise was to achieve as low a touch-down speed as possible. During the flare, power was added progressively as the nose was raised, the aircraft being held off as long as possible. This process required full visual concentration on the runway, since power had to be added largely by guesswork and "feel." If power was added too quickly, or too much, the aircraft could "balloon" off and speed would not fall off as required, leading to a prolonged float. The lowest touch-down speed that was achieved was about 85 knots.

The real difficulty here is in the float which is the result of applying power to achieve a low touch-down speed. Two possible solutions were examined. To produce a low touch-down speed at a given point on the runway, the earlier approach path had to be aimed at a point well short of the chosen point finishing with a near-level, power-on float. This technique was of doubtful value as far as the overall landing distance to rest from the conventional 50 ft. screen was concerned and useless for present carrier deck landing methods.

The other solution was to dispense with the float and most of the flare and to fly the aircraft almost on to the ground with only a slight check of the vertical velocity. Very little speed was lost during the check and a low touch-down speed implied a low approach speed. For accurate control of the touch-down point this was the most effective technique. The landing (with jets deflected), shown in Fig. 17, was of this type, although the height record is rather inaccurate near the ground.

To summarise the pilots' difficulties, they were concerned with (*a*) the high thrusts required for low speed approaches, leaving little in hand for over-shooting, (*b*) the uncertainty as to how much the thrust could be or should be reduced in order to steepen the glide path, and (*c*) the cross-coupling between throttles and elevator in controlling both speed and glide path. This last item is difficult enough to explain even when one has all the quantitative data. Even the comparative time histories in Fig. 17 do not show how the two controls were used. The pilots found it very difficult to describe their techniques in words. It is probably significant that the pilot who seemed to have least difficulty in controlling the aircraft had had experience in flying a jet-borne V.T.O. aircraft. Two-handed control is a knack which can be learned but is difficult to describe.

14. Instrument Aids to Control on the Approach

Fairly late in the programme the aircraft was fitted with a standard "Safe Flight" landing speed indicator. This device consisted of a small spring-loaded "tongue" which extended from the wing under-surface, at about 2 per cent of the chord from the leading edge, *i.e.* in the region of the stagnation point. The position of this "tongue" was displayed on a dial in the cockpit, the scale having no figures but simply a white centre portion, changing to red at one end and green at the other. The detector was so adjusted that, at about 10 degrees incidence, the pointer was in the centre of the scale. Increase in incidence moved the pointer towards the red zone, while decrease in incidence moved it into the green zone. The chosen datum incidence of 10 degrees was appropriate to 120 knots with jets straight and also to 95 knots on the approach with the jets deflected.

In flight, the pilot had simply to keep the pointer as near as possible to the centre of the white band and he could then be sure that he was flying at a safe incidence whether the jets were deflected or not. This instrument proved to be of considerable help in maintaining the pilot's peace of mind, particularly when changes in thrust were required. The instrument was placed as near as possible to the pilot's line of sight and turned on its side so that the pointer moved vertically in a natural sense. Once confidence had been acquired in it, it could replace the air speed indicator as the pilot's primary instrument reference on the approach.

The actual position of the pointer on the scale was also recorded by a small cine camera in the cockpit; a calibration of the instrument is shown in Fig. 21, the two

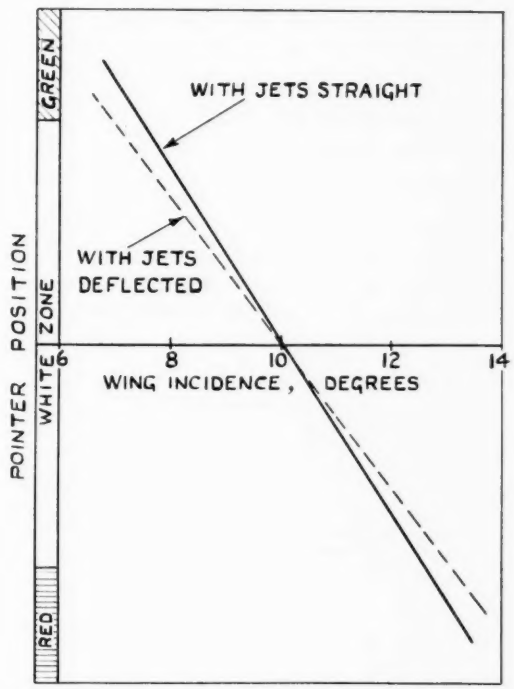


FIGURE 21. Calibration of landing speed indicator.

lines indicating that the instrument was less sensitive at the lower air speeds obtainable when the jets were deflected. This difference could be ignored as far as the pilot was concerned. The white band here represents an incidence variation of ± 3 degrees, but the width of the band was adjustable and the final setting was a compromise between close control and over-sensitivity. Roughly twice the sensitivity shown in Fig. 21 was found to be the optimum.

It was concluded that an instrument of this type (*i.e.* displaying incidence to the pilot) could be very useful on an aircraft in which part of the lift is dependent on engine thrust.

15. Lift, Drag and Thrust Measurements

15.1. Notation

C_D aircraft drag coefficient = (aircraft drag)/(qS)
C_{DM} momentum drag coefficient = (momentum drag)/(qS)
C_L aircraft lift coefficient = (wing lift)/(qS)
C_{TG} gross thrust coefficient = (gross thrust)/(qS)
q dynamic pressure = $\frac{1}{2}\rho V^2$ (lb./ft.^{2})
S wing area (ft.^{2})
V air speed (ft./sec.)
W aircraft weight (lb.)
α wing incidence (degrees)
γ glide path angle (degrees) (positive downwards)
θ jet deflection angle (degrees), relative to engine axis

15.2. Equilibrium conditions

When the aircraft is established on a steady straight glide path, the forces upon it are as shown in Fig. 22 and equilibrium conditions lead to the following two equations, referring respectively to the forces normal to and along the flight path (with the undeflected thrust line, *i.e.* the engine axis, inclined two degrees nose-down relative to the wing chord line on this aircraft).

$$C_L + C_{TG}\sin(\alpha+\theta-2) = W\cos\gamma/(qS) \quad . \qquad (1)$$

$$C_D + C_{DM} = W\sin\gamma/(qS) + C_{TG}\cos(\alpha+\theta-2) \qquad (2)$$

We have thus only two equations and four unknown quantities—lift, drag, thrust and deflection angle. The first assumption made, namely, that the wing lift and drag coefficients could be assumed unchanged when the jets were deflected (at a given incidence), gave values for the thrust and deflection angle which made it obvious that the thrust would have to be measured directly in flight.

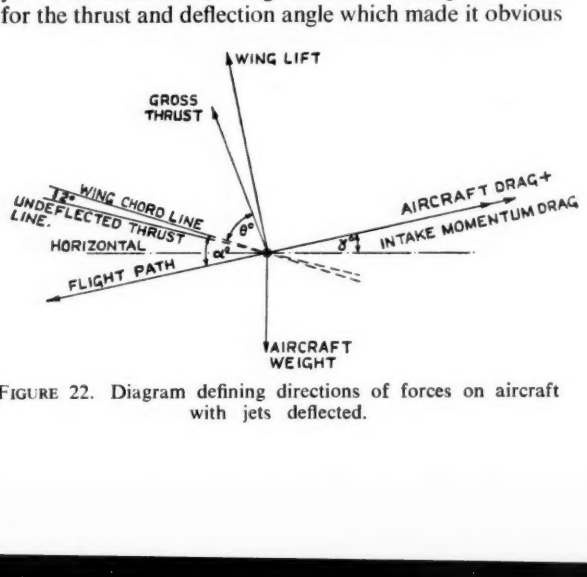


FIGURE 22. Diagram defining directions of forces on aircraft with jets deflected.

Even when this was done, with results described later, one of two possible assumptions had to be made depending on whether the lift and drag coefficients, or the deflection angle, were required. It might be assumed that the deflection angle, as determined by the bench test described in Part I of this paper, applied in flight. Alternatively, one could assume that, to find the actual deflection angle, the aerodynamic lift/drag polar was unaltered when the jets were deflected. Either assumption is open to objection. The first assumes a result which we should be trying to measure in flight. The second is equally serious in that if there were, for example, a loss in lift due to jet interference, the resulting distortion of the spanwise loading might alter the induced drag in addition to causing a possible change in profile drag due to the presence of the jet at a large angle to the air flow. This second assumption does not entirely mask all interference effects, since it does reveal changes in lift and drag at a given incidence, but it masks changes in drag at a given wing lift. Interpretation of the results is thus ambiguous, but no way out of the difficulty could be found.

15.3. Results of lift and drag measurements

The partial glide flight tests produced sets of values of glide angle and incidence at various forward speeds and weights, both with engines idling and with jets straight and deflected at 11,800 r.p.m. With the measured gross thrust and momentum drag, the results were reduced to the forms shown in Figs. 23 and 24.

The two lift curves in Fig. 23 show that there is a significant loss in lift at a given incidence when the jets are deflected. Two sets of points define the lower line, corresponding to the two assumptions mentioned, but the difference between them does not warrant the drawing of two separate lines. The variation of the loss in C_L with incidence is shown in the upper diagram of Fig. 25.

The assumption of an unchanged lift/drag relation when the jets are deflected enables an effective deflection angle to be derived and this result is also shown in Fig. 23. Although a small variation with incidence is apparent, the mean value is close to the assumed 61°.

Two drag curves for the landing configuration are shown in Fig. 24. The lower (dotted) line refers to the undeflected case and is the one which is assumed to apply also to the deflected case when the effective deflection angle is being derived. The upper line is

TABLE I

Flaps	*Under-carriage*	*Jets*	*Induced drag factor, k*	*Profile drag coefficient C_{D0}*
Up	Up	Straight	1·06	0·023
Down	Down	Straight	2·42	0·100
Down	Down	Deflected	2·38	0·119

Note: (*i*) by definition, $C_D = C_{D0} + kC_L^2/(\pi A)$, where A is the aspect ratio.
(*ii*) data derived for the "jets-deflected" case are based on an assumed constant deflection angle of 61°.

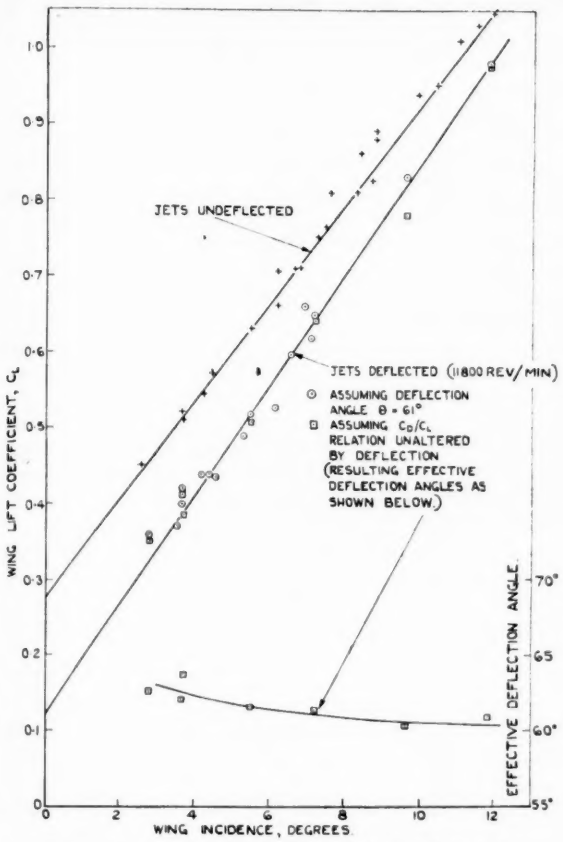


FIGURE 23. Lift curves with jets straight and deflected.

obtained when a constant deflection angle of 61 degrees is assumed.

The magnitude of the interference effect on drag depends upon which of these assumptions is made. Fig. 25 summarises the interference data for both lift and drag and the lower diagram shows that there is generally a reduction in drag coefficient at constant incidence, which is small when a constant deflection angle is assumed, becoming about 0·015 with the alternative assumption. At constant C_L the first assumption leads to an *increase* in C_D of 0·015, while the second necessarily gives a null result.

The slopes of the lines in Fig. 24 give the induced drag factors for the various configurations, while the zero lift intercepts give the profile drag increments. These results are given in Table I.

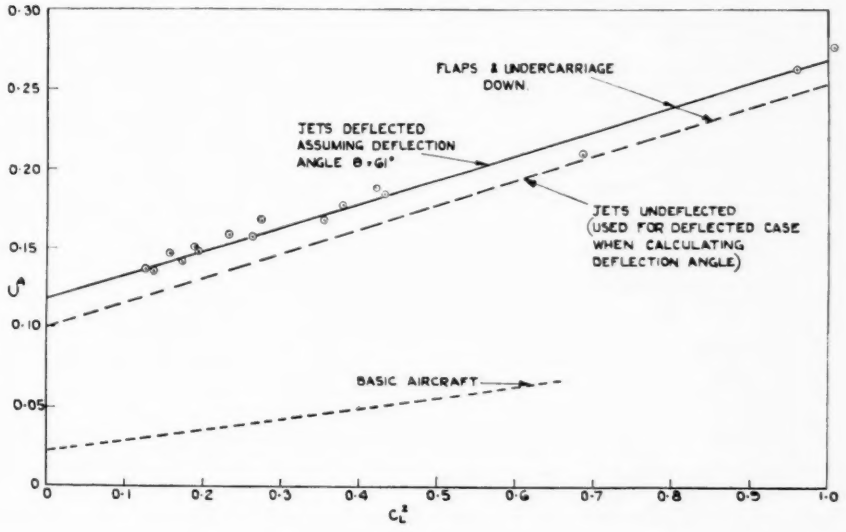


FIGURE 24. Summary of flight measurements of aircraft drag.

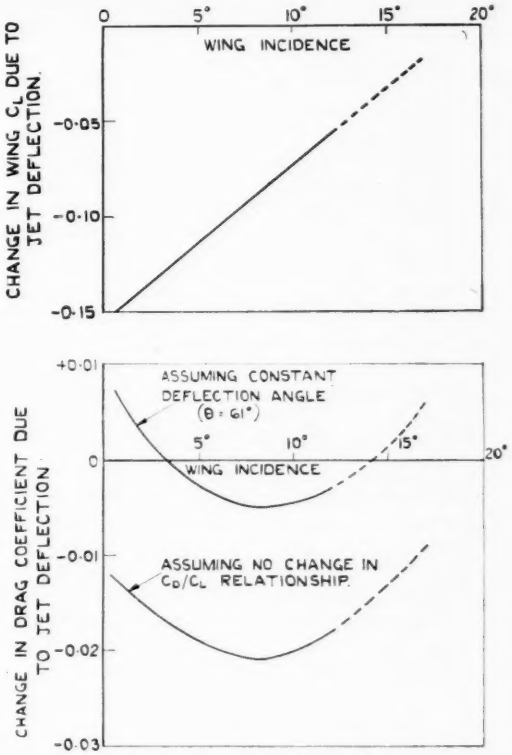


FIGURE 25. Summary of jet interference effects.

It must be concluded that the interference effect on drag is uncertain, while the effect on lift is reasonably well established. Tunnel evidence suggests that the loss in lift is probably due to induced suction forces on the undersides of the nacelles.

15.4. PERFORMANCE DIAGRAMS

Using the measured lift and drag coefficients and the measured thrust, with the assumed 61 degree deflection

angle, a chart can be drawn relating the flight path angle to air speed, for constant wing incidence or constant throttle setting. Fig. 18 is such a chart for sea-level conditions with 25 per cent fuel remaining, thrust being expressed as percentages of the full-throttle value.

This diagram should be compared first with the similar picture of Fig. 26 which is the slightly hypothetical case of zero total deflection angle (*i.e.* with the thrust always along the flight path). The differences are thus due only to the deflection angle and show that roughly double the thrust is required in the deflected case to achieve a given glide angle, although, of course, the corresponding speeds are lower. Whereas with undeflected thrust, the speed for minimum glide angle (about 120 knots) does not vary with thrust, in the deflected case this speed has fallen to about 100 knots at full throttle. The minimum glide angle occurs at a lower incidence as the deflected thrust is increased.

By cross-plotting at constant glide angles on Figs. 18 and 26, the percentages of full throttle thrust required to hold a given glide path may be obtained, as in Figs. 19 and 27 for the deflected and undeflected cases respectively. The significant difference between the two is the much more rapid development of unstable conditions as speed falls below that for minimum thrust, in the deflected case.

The dotted portions of these curves (for incidences above 12 degrees) are somewhat uncertain extrapolations. An estimated stall limit of 17 degrees has been used. This leads to an absolute minimum speed at full throttle of 61 knots, which, as mentioned earlier, was only ever approached on one occasion. It corresponds to a gross maximum lift coefficient of 3·24, which is 2·4 times the zero thrust value.

Finally, Fig. 20, which shows the effect of weight on performance, is included mainly to illustrate the sort of difficulty which the pilots met in the course of a flight in assessing the optimum approach conditions.

16. Thrust Measurements

The direct measurement of the deflected thrust was not part of the original test programme, but was made necessary by the failure of the performance measurements to yield values of the thrust and deflection angle which could be reconciled with the test bed values.

Discussion with the N.G.T.E. showed that the only worthwhile method of thrust measurement involved the use of a Pitot-static rake across the deflected jet nozzle. The method had already been used in flight, by both the N.G.T.E. and the A. and A.E.E., but never on a jet with such a distorted flow pattern.

Measurements were made on the port engine in steady flight at a range of air speeds up to 250 knots at the engine speed (maximum continuous—11,800 r.p.m.) used during the performance measurements. Records were also taken at zero speed, using a special engine-running pit at Farnborough, for direct comparison with the test bed figures. Each measurement was repeated for each of the four rake positions. In addition, for a ground test only, the rake was fitted temporarily across the straight-through jet pipe of the same engine, to obtain the undeflected thrust.

The undeflected thrust was also measured by the simple device of tethering the aircraft to a large spring balance, adding a correction for the friction of the undercarriage. This method only gave the average thrust of the two engines, of course.

The Pitot-static measurements were first plotted according to their diametric positions and contours could be drawn of the type shown in Fig. 28 which is for the deflected case at 250 knots. Computation was easier if the pressures were replotted against circumferential position at fixed radii, although Fig. 28 gives a clearer visual picture of the flow distributions.

The gross thrust, T_G, was determined by graphical summation over annular elements of area from the equation:

$$T_G = \Sigma \rho V^2 \,.\, \delta A + \Sigma\,(p_e - p_a)\,.\,\delta A$$

where ρ density (slugs/ft.^{3}) in the jet at the nozzle
V jet exit velocity (ft./sec.)
δA element of nozzle area (ft.^{2})
p_e static pressure in the jet at the nozzle (lb./ft.^{2})
p_a ambient static pressure (lb./ft.^{2})

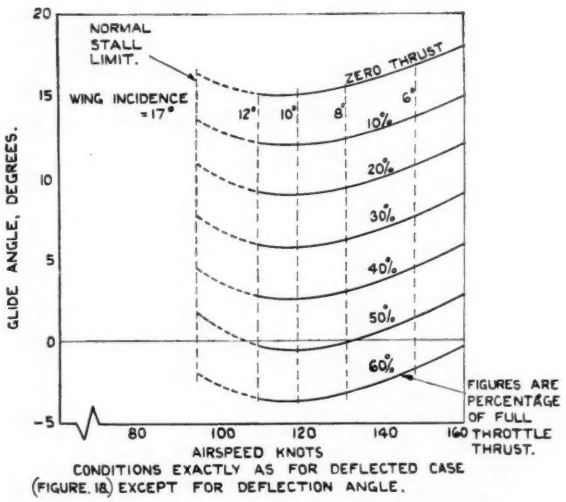


FIGURE 26. Performance with zero total deflection angle.

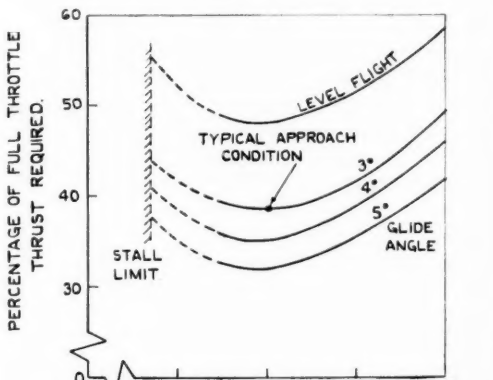


FIGURE 27. Thrust required for various glide paths with zero total deflection angle.

Although one-dimensional flow theory predicts that the second term in this equation should be zero at the overall nozzle pressure ratios developed in these tests (*i.e.* for an unchoked nozzle), it was, in fact, not zero. This is scarcely surprising considering the non-uniformity of the pressure readings across the jet.

The first term in the equation was summed over discrete annular elements using the recorded Pitot and static pressures and jet-pipe temperatures, and the derived thrusts were corrected to standard sea-level conditions, and for the estimated drag of the rake itself. Intake momentum drag could also be obtained from the derived mass flow. Fig. 29 summarises the thrust measurements in flight and also compares the various static thrust measurements.

There was no measurable loss in thrust due to deflection at constant engine speed, as measured by the rake, although the jet-pipe temperature was about 20° C higher when deflected. The spring balance figure agrees remarkably well with the rake data, especially since there was flight evidence that the port engine was giving slightly more thrust than the starboard engine. The difference between the flight data and that obtained by the N.G.T.E. may be due simply to the detail differences between the engines and deflector losses on which the two sets of measurements were made.

The data of Fig. 29 form the basis of the corrections which were applied to the performance measurements to enable the wing lift and drag coefficients to be found.

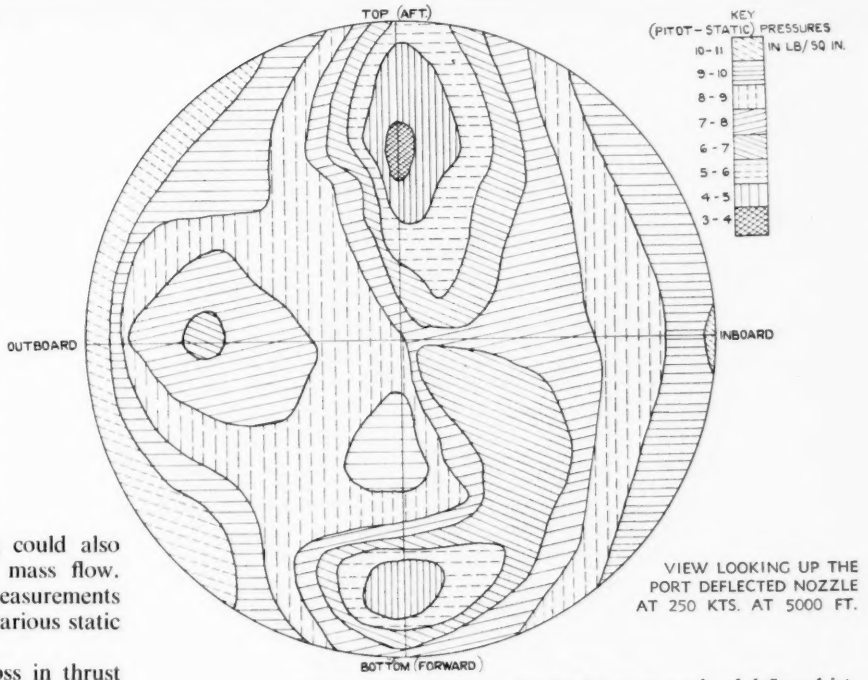


FIGURE 28. Pressure distribution over nozzle of deflected jet.

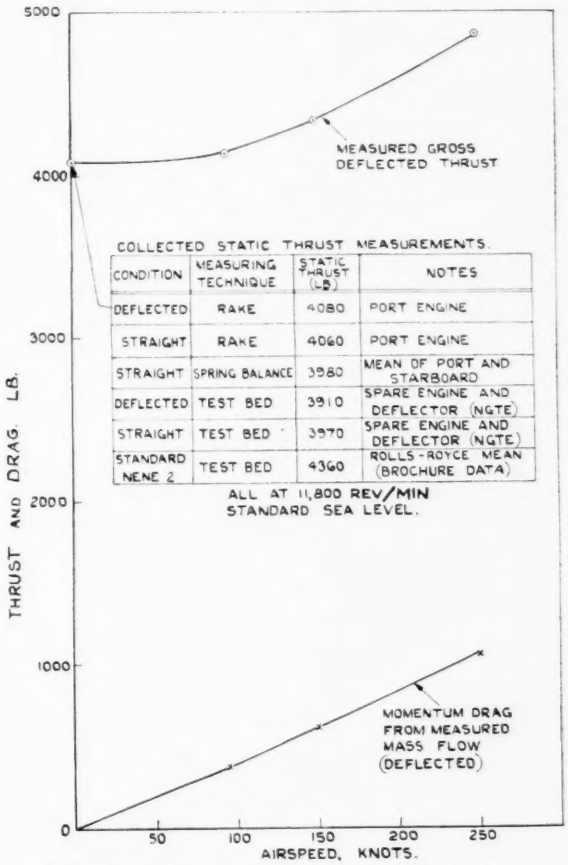

5000
4000
3000
2000
1000
0
THRUST AND DRAG, LB.
MEASURED GROSS DEFLECTED THRUST
COLLECTED STATIC THRUST MEASUREMENTS.
CONDITION | MEASURING TECHNIQUE | STATIC THRUST (LB) | NOTES
DEFLECTED | RAKE | 4080 | PORT ENGINE
STRAIGHT | RAKE | 4060 | PORT ENGINE
STRAIGHT | SPRING BALANCE | 3980 | MEAN OF PORT AND STARBOARD
DEFLECTED | TEST BED | 3910 | SPARE ENGINE AND DEFLECTOR (NGTE)
STRAIGHT | TEST BED | 3970 | SPARE ENGINE AND DEFLECTOR (NGTE)
STANDARD NENE 2 | TEST BED | 4360 | ROLLS-ROYCE MEAN (BROCHURE DATA)
ALL AT 11,800 REV/MIN STANDARD SEA LEVEL.
MOMENTUM DRAG FROM MEASURED MASS FLOW (DEFLECTED)
50
100
150
200
250
AIRSPEED, KNOTS.

FIGURE 29. Flight measurements of gross deflected thrust.

17. Conclusions from the Flight Tests

This flight experiment showed that the stability and control of the Meteor were adequate for test flying at speeds down to 70 per cent of the normal (power-off) stalling speed. The main piloting problem occurred on the landing approach. No longer was it possible for the pilot to separate out the actions of throttles and elevator as means of controlling speed and glide angle. A new technique of using the throttle had to be mastered, since this control affected both the longitudinal and the normal force on the aircraft at the same time. It was relatively easy to gain height or speed, since application of power always led to an increased feeling of safety. Conversely, any correction involving a reduction in power worried the pilot in case it was overdone. Pilots found it difficult not to think of the increase in stalling *speed* when power was reduced as a real increase in the danger of exceeding the stalling incidence. Further, since approaches were possible at speeds below that for minimum glide angle, the usual

difficulties in flying below the minimum drag speed were present and to a more marked extent than usual.

Analysis of the results of the performance measurements was not entirely satisfactory. In spite of having measured the deflected thrust in flight, it was not possible to determine both the actual deflection angle and the interference between the jets and the aerodynamic forces on the wing at the same time. One had to be assumed in order to derive the other. The evidence does suggest a loss in lift with not much change in drag, but the drag change in particular is somewhat uncertain.

The experiment has provided valuable experience in the control and handling of a class of aircraft which we may expect to encounter more frequently in future. If for this reason alone, it has fully justified itself.

ACKNOWLEDGMENTS

Both authors are indebted to the numerous members of the Staffs of the National Gas Turbine Establishment and the Royal Aircraft Establishment who contributed to the progress of this experiment. Particular thanks are due to the test pilots of the Aerodynamics Department, R.A.E., who approached the new problems on this aircraft with their usual enthusiasm.

The views expressed are the authors' own, and do not necessarily represent those of the Ministry of Supply.

REFERENCE

1. LEAN, D. and EATON, E. F. (1957). The Influence of Drag Characteristics on the Choice of Minimum Approach Speeds. AGARD Report No. 122, May 1957.

APPENDIX I

WEIGHT ANALYSIS OF JET DEFLECTOR INSTALLATION

Description	*Weight* LB.	OZ.		
Valve body	105	0		
Straight vane	55	0		
Curved vane	53	8		
Stubshaft (4 off)	17	12		
Bearings and cartridge (4 pairs)	4	0		
Shroud (valve body)	8	0		
			243	4
Valve body extension	30	0		
Shroud (valve body extension)	7	8		
			37	8
Straight jet pipe and nozzle	56	8		
Shroud (straight jet pipe)	18	11		
Flange (attachment to valve body)	6	8		
			81	11
Deflected jet pipe and nozzle	32	8		
Valve locking mechanism	15	0		
Studs, bolts, washers, and so on	1	0		
TOTAL	410	15		

APPENDIX II

TEST SCHEDULE FOR FLIGHT CLEARANCE TEST

The object of this test was to provide initial flight clearance for the exhaust unit, deflector and jet pipes which were special features of the Meteor Jet Deflection Project.

TEST PROCEDURE

Stage 1—*Incremental cruise and slow running*

(*a*) Five accelerations and decelerations from slow running to maximum speed.

(*b*) Four and a half hours at incremental cruising speed consisting of thirty minutes at each of the following speeds:—

(1) 6,250 r.p.m. (4) 8,500 r.p.m. (7) 10,750 r.p.m.
(2) 7,000 r.p.m. (5) 9,250 r.p.m. (8) 11,500 r.p.m.
(3) 7,750 r.p.m. (6) 10,000 r.p.m. (9) 12,250 r.p.m.

(*c*) Half an hour at slow running speed.

Duration 5 *hours.*

Stage 2—*Thirty* 15-*minute cycles*

(*a*) Accelerate from slow running to 12,500 r.p.m.
(*b*) Run for 5 minutes at 12,500 r.p.m. (straight jet).
(*c*) Decelerate to approach idling (5,500 r.p.m.).
(*d*) Run for 2½ minutes at 5,500 r.p.m.
(*e*) Operate valve to deflect at 5,500 r.p.m.
(*f*) Accelerate to 12,500 r.p.m.
(*g*) Run for 5 minutes at 12,500 r.p.m. (deflected jet).
(*h*) Decelerate to slow running (2,500 ± 100 r.p.m.).
(*j*) Operate valve to straight-through.
(*k*) Run for 2½ minutes at 2,500 ± 100 r.p.m.

Repeat the above until 30 such cycles have been completed.

Duration 7½ *hours.*

Stage 3—2½ *hours non-stop cycle*

(*a*) Five accelerations and decelerations from slow running to maximum speed.
(*b*) Run for 15 minutes at take-off (12,500 r.p.m. and 690°C jet pipe temperature) with straight jet.
(*c*) Run for 2 hours at cruise (11,800 r.p.m. and 595°C jet pipe temperature) with straight jet.
(*d*) Decelerate to half thrust (approximately 10,300 r.p.m.) and deflect jet.
(*e*) Run for 15 minutes at take-off (12,500 r.p.m. and 733°C exhaust cone temperature) with deflected jet.

Duration 2½ *hours.*

Stage 4—2½ *hours non-stop cycle*
Repeat Stage 3.

Duration 2½ *hours.*

Stage 5—2½ *hours non-stop cycle*
Repeat Stage 4.

Duration 2½ *hours.*

Stage 6—2½ *hours non-stop cycle*
Repeat Stage 5.

Duration 2½ *hours.*

Stage 7—5 *hours at maximum climb r.p.m.*

(*a*) Run at half thrust (approximately 10,300 r.p.m.) and operate deflector valve six times.
(*b*) Run for 5 hours at maximum climb speed (12,200 r.p.m. and 651°C jet pipe temperature) with straight jet.

Duration 5 *hours.*

Total scheduled running 27½ *hours.*

DISCUSSION

D. L. Hollis Williams (Technical Director, Westland Aircraft Ltd., *Fellow*): He conveyed the greetings of the Yeovil Branch to the Christchurch Branch, on the occasion of its first main lecture and complimented the lecturers on a first-class technical description of an interesting experiment which started in mid-1953.

It was an extremely crude experiment in that the Meteor aircraft selected was really unsuitable because of its asymmetry. The Rolls-Royce Nene engines had had to be mounted in a very far forward over-hung position, to give a deflected jet thrust in correct relation to the aircraft centre of gravity, and no great time was spent in providing the lightest possible equipment. They finished up with an aircraft which had very little disposable load, and yet the experiment was successful.

It was the sort of experiment he liked, which had no magic in it—it was a simple case of cause and effect, and because of that he was amazed that practical use had not been made of this simple experiment.

He thought this was the first full technical disclosure of the effect of jet deflection, which was kept on the Secret List until March 1955 when Sir Arnold Hall, then the Director of the Royal Aircraft Establishment, said at a lecture that such experiments were going on.

They certainly realised by mid-1955 that this was a most important development, but at the time, his Company was undergoing a change in technical policy by deciding to concentrate on helicopters, and so they did not carry on with this development.

They started the deflected jet experiments in June 1953 and the first flight was made by S./Ldr. de Vigne, a Westland Test Pilot, in February 1954 and it was handed over to Farnborough in August 1954.

He was sure that if an aircraft were designed *ab initio* for jet deflection it would be possible to get a very simple installation with all the devices contained in one short length of jet pipe.

The lecturers said that they were confining themselves to this particular application of lift augmentation, but he would like to know how, in their opinion, it compared, both in simplicity and effectiveness, with lift augmentation by means of blowing.

J. R. C. Fearon (Rolls-Royce, *Associate Fellow*): If an aircraft were to be designed from the start to incorporate jet deflection for landing, would any modifications be required to present engine handling characteristics, particularly quicker thrust response to throttle movement? Would the lecturers care to comment further on the mechanism whereby with jets deflected there was a loss of wing lift. (Fig. 23.)

While describing Fig. 17, the lecturer stated that with jets deflected the aircraft was flown right down onto the deck, but the figure showed that engine r.p.m. was reduced about two seconds before touchdown at a height of approximately 50 ft. This seemed surprising, especially as he would expect the deflected thrust to be reduced at an even greater rate than the r.p.m. Was there some ground effect giving assistance below this 50 ft. height point?

He was interested that Fig. 18 showed a decrease in minimum drag air speed with increase in deflected thrust. Did the decrease line up with the following simple explanation? The drag coefficients, profile and induced, fixed the aircraft minimum drag lift coefficient, and therefore, after subtracting the weight supported by the deflected jets, the air speed required for the wing to support the remainder of the weight at the minimum drag lift coefficient was calculated.

Finally, if he might be permitted, a small criticism. The formula quoted in Section 16 to obtain gross thrust, *i.e.* deflected jet thrust, made it necessary to measure the jet pipe temperature distribution. It was possible to obtain gross thrust purely from a knowledge of Pitot and static pressure distributions; the effect on thrust of changes in γ due to changes in temperature being negligible. A knowledge of the temperature distribution was only necessary to obtain mass flow and hence net thrust. Therefore an analysis of the rake result by this method would have eliminated the necessity of measuring the third parameter, temperature.

A. R. Howell (National Gas Turbine Establishment, *Fellow*): This paper was important because they would not get practical VTOL and STOL aircraft until a great deal more of this kind of work was done and reported. He wondered whether or not the control problems described applied to the transition stage between hovering and normal flight for VTOL aircraft?

Coming from the N.G.T.E., his own interests were more on the engine side. The thrust deflector used for this experiment was a two position one, arranged for straight through operation or for deflection through 60°. Mechanically the deflector was very successful and they seemed to have made a wise choice for this particular investigation. However there were other types of thrust deflectors that were worth considering, for example, a deflector where the angle of thrust could be changed continuously. Again, one could have a deflector with two fixed jets at zero and 60° angle, but the jets were themselves adjustable in flow area in order to vary the vertical and horizontal components of thrust relative to one another. Would some control problems be simplified if they had more variability in the thrust and its angle?

On weight the lecturers quoted a value of 292 lb. for the deflector alone so that for a thrust of 4,800 lb., this became a specific weight of about 0·06 lb./lb. thrust, which was not really low enough. More variability of the jets would tend to increase the weights, so the lightest constructions would be needed.

The jets were very close to the ground, were any ground effects noticed or were the engines shut off before the aircraft got very close to the ground? The cross-coupling between throttles and elevator in controlling both speed and glide path seemed to be particularly difficult. He felt that the engine throttle must in the long run be used as a secondary and not a primary control; and so the aircraft must be controllable, with fixed throttle setting, by means of adjustable thrust angle and by some variable drag device.

The results obtained emphasised the detrimental effects of high ambient temperatures and increases in gross weight and altitude, and these were problems which were very much associated with VTOL and

STOL aircraft. With the Meteor experience behind them how did the lecturers feel on the question of deflected jets to augment lift as compared with separate lifting and propelling engines?

A. Pierce (de Havilland (Airspeed) Division): The lecturers referred to the difficulties of flying at speeds below the minimum drag speed. Was this a serious problem, or was it a "trick" which could be learned with practice? Why were approaches not made at speeds below the minimum drag speed with the "undeflected" aircraft?

Mr. Lean: Flight at speeds below minimum drag speed introduced another source of error—a speed divergence—which demanded more of the pilot's attention than usual and was always undesirable. This divergence was tolerable to a certain extent in some circumstances, for example, when a short, visual approach was being made, but there was a definite limit to it. It was more difficult than usual to deal with on the deflected-jet Meteor because the throttles were more effective in varying the lift force than in controlling the forward speed.

The approach speed of the "undeflected" Meteor was probably limited by proximity to the stall, before the speed divergence state was reached. In other words, minimum drag speed considerations did not influence the choice of approach speed in this case.

J. K. Moakes (A. & A. E. E., Boscombe Down): In view of the obvious benefits arising from the reduced approach speed—reduced runway length, for example —why was jet deflection not more widely adopted?

Mr. Lean: One could think of many possible reasons. Probably the most important was the requirement that the deflected thrust line must lie through, or close to, the c.g. This largely dictated the design of the aircraft. It made any retrospective modification very difficult or even impossible, and on a new design it might conflict strongly with other requirements. Unless there was an over-riding need to achieve a very low approach speed, one supposed that it was easier to use the long runways that already existed.

Also, unless a fairly high thrust/weight ratio was available—probably higher than that available on conventional jet transport aircraft today—then the benefits might scarcely compensate for the extra complication.

R. K. Page (Chief Technician (Projects), Folland Aircraft Ltd., *Associate Fellow*) *Contributed*: Although the aircraft selected for this experiment was generally quite suitable for the installation of the experimental feature, there were still found, on closer investigation, to be some fairly major changes indirectly required, *e.g.* the larger and more effective controls, the stronger undercarriage. From association with this project in its earlier stages he would like to emphasise the advantage from the time aspect of being able, with the Meteor, to make these changes by the simple process of ordering from R.A.F. stores instead of by partial or complete re-design and manufacture. This facility would not have been available if the aircraft had been a "one-off" prototype or in limited production and not a well-proved service type with several variants.

He had two particular points from the flight tests: (*i*) was any use made of the air brakes as a glide control on the approach—or possiby those on the Meteor were not suitable for variable application? (*ii*) Ground effects, such as lift and trim change and airframe heating were not mentioned, were they appreciable?

The paper showed that in itself this experiment with jet deflection *was* "by and large" a successful one, *i.e.* the scheme "worked" mechanically, the quantitative performance results were in fair agreement with those predicted, and the aircraft had tolerable handling qualities, which could probably be improved upon in a future installation as a result of the experience gained. *But*, was this principle of jet deflection an attractive one for application to new designs of operational aircraft? In spite of initial enthusiasm, one was reluctantly led to answer "No." The reasons were given in the paper; first, as stated in Part I, Section one, the line of action of the deflected thrust must pass very close to the aircraft c.g., and this at once placed a severe restriction on the whole layout of the aircraft because in practice it meant that the engines had to be installed on, or forward of, the c.g. and, if severe exhaust heating and vibration troubles were to be avoided, fairly well outboard, whereas the optimum position in the types most likely to be concerned, namely large fighter or strike aircraft, was usually with the engines appreciably behind the c.g. and near the centre line. One type in which this objection would not apply was, of course, the transport with wing-podded engines, such as the Boeing 707, but here the second objection was likely to apply.

The second point was that it was a great advantage for any high-lift device to be applicable to both landing and take-off (in general) and in this respect jet deflection, as here considered, with total thrust appreciably *less* than aircraft weight failed to be of much advantage. (The poor climb away was mentioned in the paper.) The optimum angle at which the favourable lift component predominated over the unfavourable cosine effect *reducing* the forward thrust was in general only about $\frac{1}{4}$ to $\frac{1}{2}$ that suitable for landing, so that a two or multi-position nozzle was essential and even with this optimum angle the reduction in take-off distance was only appreciable for aircraft with thrust/weight ratios high enough, 0·6 and over, such that the take-off was likely to be acceptable anyway, being considerably less than the landing distance. The authors did not commit themselves to any prophesies, so did they agree with these rather discouraging conclusions?

Mr. Ashwood's and Mr. Lean's Replies

Mr. Hollis Williams: They thanked Mr. Hollis Williams for the part which he and his Company played in producing a really useful, successful, research aircraft. It gave remarkably little trouble to operate, and it took considerable punishment on occasions.

The conversion was undertaken to provide a research aircraft and not an operational one-and no great effort was made to keep the weight of the deflector units down, mechanical reliability being considered of prime importance.

Mr. Hollis Williams asked how jet deflection com-

pared with flap blowing. The aerodynamicist was tempted to say that someone (*i.e.* someone else) must do detailed project studies of rival schemes before a true comparison could be made. However, it must be remembered that on this aircraft they achieved a reduction in approach speed of 20 to 25 knots. To have achieved this by flap blowing, with virtually no trim change one would have to be very clever indeed.

Mr. Fearon: An improvement in the rate of thrust response would be desirable and a practical way of achieving this, although at some increase in weight, would be by the use of variable propelling nozzles.

The observed loss in wing lift was of the order expected on the basis of simple wind tunnel tests. Pressures were measured on the undersurface of a model nacelle with representative flow, but no pressures were measured on the wing itself. Basically, one would expect a slight increase in pressure ahead of the jet and a reduction behind it. The variation of the loss in lift with incidence had not, so far, been explained.

The height record in Fig. 17 must be treated with caution—it was indicated pressure height only. The reduction in thrust two seconds before touchdown was in the nature of a correction, rather than a "cut" of the power—in fact, the engine speed at touchdown was still quite high. They found no significant ground effect at all—certainly not at 50 ft.

Mr. Fearon's deduction of the decrease in minimum drag speed with increased thrust was substantially correct, to a first approximation. However, Fig. 18 showed that the minimum drag air speed did not occur at the same incidence as thrust was increased, because the incidence, and hence the deflection angle, varied as speed varied. This complicated the calculations a little.

The measurements in the deflected jet described in Section 16 were made to determine the net thrust in flight, and it was for this reason that temperature had to be measured as well as Pitot and static pressures.

Mr. A. R. Howell: The control problems which they described were similar to those which one would expect at the "high-speed" end of the transition of a V.T.O.L. aircraft, if, in that aircraft, changes in engine power affected both lift and thrust. Control would probably be easier on the V.T.O.L. aircraft if an independent lifting system were used.

They had examined theoretically the relative merits of fixed deflection angle, variable deflection angle, and separate lift and thrust engines. With some reasonable assumptions about overshoot performance, they found that the variable deflection angle system allowed the biggest reduction in approach speed and the separate lift and thrust engines the least reduction, for a given available thrust/weight ratio. Probably, from the pilot's point of view, this order of preference would be reversed. This was the root of the question for which one would first seek an answer on a simulator. Independent controls for lift and thrust were what the pilots said they would have liked.

The throttle was a very powerful control and one felt that there would be pressure applied to allow it to be used as a primary control.

They had no trouble with ground effect on a normal landing, nor even on the occasion when the aircraft was flown down the length of the runway with the wheels just clear of the ground, to study this very point.

They agreed with Mr. Howell that the specific weight achieved with the butterfly valve type of deflector unit was not low enough, particularly in view of the fact that values in the region of 0·05 lb./lb. thrust appeared possible for complete lifting engines. However weight was not the only criterion and the volume occupied by the "lift producer"—be it a separate engine or a deflector unit—was a potent factor in determining the choice. Lighter and simpler forms of deflector could be developed and one such device had already been successfully demonstrated on the test bed.

Mr. R. K. Page: The ability to modify the aircraft by the use of standard parts was most valuable, and undoubtedly saved time and expense.

Air brakes were used on odd occasions to reduce an excessive air speed, in the same way that the flaps were sometimes used in an attempt to improve speed control. Neither device was satisfactory for it to have been used as a primary flying control.

They found no appreciable ground effects in normal operation, since the throttles were closed as soon as the wheels were on the ground. Similarly, there were no heating problems, apart from slight trouble with ventilation of the rear part of the nacelle, with the jets deflected, due to induced reverse flow.

Mr. Page was rather pessimistic in his views on the applications of jet deflection. It was certain that if the device were to be used, the aircraft had to be largely designed around the deflected nozzles. One could envisage layouts in which engines in nacelles might be placed well forward and inboard, although the exhaust heating and vibration problems might be severe.

One advantage of jet deflection, not so far considered, lay in the fact that it allowed the engines to run at high thrust on the approach. They had, thus, a more effective source of compressed air for use, for example, with blown flaps, giving a further increase in lift. This was attractive when one considered a fairly "clean" aircraft, such that normally insufficient thrust was used on the approach to allow blown flaps to work to their best advantage.

As usual, a fairly high installed thrust/weight ratio was needed to make this work. The sine of the optimum deflection angle for minimum ground run was equal to the thrust/weight ratio, so that, as Mr. Page said, a quite small deflection angle was all one could tolerate for take-off. A two position deflector might be worthwhile in this case, but in general they did not see much advantage in such a scheme, since take-off was less likely to be critical than the landing.

Although jet deflection was not the panacea for all landing problems, it was worth serious consideration on a new layout when slow approach speed was at a premium and the requirements for high speed relatively modest. However, one could not assert that any projected aircraft should or should not have jet deflections until a more-or-less detailed study had been made. For that reason they avoided making any prophecies.

Guided Weapons and Aeronautics

by

R. COCKBURN, C.B., O.B.E., M.Sc., Ph.D.
(Controller of Guided Weapons and Electronics, Ministry of Supply)

The 1068th lecture to be given before the Society "Guided Weapons and Aeronautics" by Dr. R. Cockburn, C.B., O.B.E., M.Sc., Ph.D., M.I.E.E., A.Inst.P., was given on 10th April 1958 at the Institution of Mechanical Engineers, Birdcage Walk, London, S.W.1. Air Marshal Sir Owen Jones, K.B.E., C.B., A.F.C., B.A., D.I.C., M.I.Mech.E., F.R.Ae.S., Vice-President of the Society presided and introduced the Lecturer. Dr. Cockburn, who was appointed Controller of Guided Weapons and Electronics, Ministry of Supply, in October 1956, was the first scientist to hold a post of Controller in the Ministry. He entered the Scientific Civil Service in 1937 at the Royal Aircraft Establishment, Farnborough, as a member of the team engaged on the development of V.H.F. communications. From 1939 to 1945 he was at the Telecommunications Research Establishment as head of the Radio Countermeasures Division. From 1945 to 1948 Dr. Cockburn was in the Nuclear Physics Division at the Atomic Energy Research Establishment at Harwell and, for a short time, at Chalk River. From 1948 until 1954 he was Scientific Adviser to the Air Ministry and then returned to the Ministry of Supply as Principal Director of Scientific Research (Guided Weapons and Electronics). He became Deputy Controller of Electronics in 1955.

Before entering Government Service Dr. Cockburn taught physics at West Ham Municipal College, London, and was engaged in research on transit time effects in valves. He was appointed O.B.E. in 1946 and C.B. in 1953 and in 1947 was awarded the American Congressional Medal for Merit for his work in the field of radio warfare.

Introduction

Although the Guided Flight Section of the Society was only established last year, there have already been a number of valuable lectures and discussions and we can rely on the Section to provide a series of authoritative papers on various aspects of design and development. It might, therefore, be timely and appropriate for the Society to consider these new weapons in a somewhat wider context; and I propose to discuss the impact of guided weapons on our concept of air power and on the field of aeronautics as a whole. I must make it quite clear that I am expressing my own views; I am not describing official policy.

During the past few years the aeronautical engineer has been facing up to a fairly drastic reappraisal of his objectives. This is not due to the failure of any of the techniques which contribute to his field. On the contrary it is the continuous and, in some cases, striking developments in technique which are revealing new concepts that must inevitably displace the old. The situation is reminiscent of the period twenty years ago when advances in structural methods and in aerodynamic knowledge led to the replacement of the biplane, which had held the field for so long, by the monoplane; or to the period ten years ago when the jet engine began to replace the piston engine.

Military aircraft have always had an important influence on aeronautical progress and the trend and pace of development has been determined in a large measure by the operational requirements of the Royal Air Force. These requirements are undergoing a radical change. One of the factors contributing to this change is the emergence of the guided weapon; but even more important and fundamental in its effect is the nuclear warhead. These two developments must be considered together if we are to appreciate the far reaching changes which are being imposed.

The small size and weight of the nuclear warhead has made possible technical advances in offensive weapons culminating in the long range ballistic missile; and its immense lethality makes such elaborate weapons economically feasible. But before examining the effect on the design of aircraft and guided weapons we should consider first the impact of the nuclear threat on strategy and on our concept of air power, since this has important consequences for the Aircraft Industry.

The Nuclear Weapon

Since the Second World War the military situation has been dominated by the increasing ascendency of the offence. Progress in offensive nuclear weapons has not been matched by corresponding advances in defensive systems and we may expect this disparity to grow. This, somewhat paradoxically, is imposing a change of strategy. For the past decade strategy has been essentially offensive; during the next it will become essentially defensive. The combination of the nuclear warhead with supersonic and, eventually, ballistic means of delivery, seemed at first to place civilisation at the mercy of the first aggressor, but this is far from being the case. On the contrary, as the threat becomes reciprocal, nuclear weapons provide the basis for a truly stable military environment. The key to this stability lies in the continued superiority of the offence over the defence; and the greater the disparity, the greater the stability.

For at least the next decade the intercontinental ballistic missile should give the offence an almost absolute superiority. Direct defence is certainly not impossible; indeed conditions can be envisaged in which interception and destruction of a ballistic missile is certainly feasible. But the ability to destroy a missile does not constitute a defence. To be effective a defensive system must cover all vulnerable areas, and must be

immediately available against a range of possible tactics. Advances in defensive techniques are unlikely to remove the vulnerability to saturation and surprise.

Under these conditions the strategic threat can no longer be used as a compulsive instrument of policy. Use of the nuclear weapon could only invoke immediate annihilation of one's own population. Between military powers possessing the means for immediate and inescapable retaliation, the strategic threat becomes neutralised. The nuclear weapon should lead not to unrestricted Armageddon but to a stalemate of mutual deterrence. This terrible instrument of war has found its own defence in the logic of retaliation. We must be careful, however, not to over-estimate the power of the deterrent. It is effective only if threat and counter-threat are reciprocal.

It is clear that an aggressor would not be restrained from attacking a country whose retaliatory weapons were limited to targets of secondary importance. It is equally true that, in the face of retaliation, attacks on secondary targets cannot be prevented by threatening the aggressor's heartland. The defensive function of the strategic deterrent extends only to objectives which are vital to survival. There will always be other areas which must be protected, but whose importance would not justify exposing vital objectives to retaliation. These must continue to be defended directly.

Moreover, the deterrent alone does not provide defence against all possible threats. No nation will invoke retaliation while there is any possibility of escaping an irrevocable decision, and minor infringements might have to be tolerated until eventually the ability to use the deterrent itself became weakened. Air defence, although on a limited scale, is still necessary to prevent minor provocations such as reconnaissance, demonstrations against public morale and even attacks on isolated military objectives.

Although the logic of retaliation removes one particularly damaging threat, it does not prevent all possibility of war. Indeed, the inhibition of major war makes peripheral wars more probable; but these should be limited in extent. It will be very much the concern of the major military powers to confine such conflicts and avoid involving themselves too deeply, but the threat of tactical nuclear weapons should act more directly to reduce the scale of war. Not only is there less need for large forces but concentration of any sort invites annihilation.

Thus, the nuclear weapon should lead to an easement in the burden of armament because of the mutual restraints which it imposes. Total war becomes an improbable catastrophe and warfare should be limited generally in scale. As a result the logistic basis of war is changing. Military strength will depend more on resources accumulated in peace than the ability to outproduce the enemy in war; and more on technical progress than on the size of front line forces. The effect of these trends on the Aircraft Industry is already apparent.

The economy of the nuclear weapon and the limitation of the scale of war which it imposes has removed the need for large numbers of offensive and defensive aircraft. Resort to nuclear weapons would certainly shorten any major war because of sheer inability to make good its ravages. There is thus little justification for maintaining capacity in peacetime solely for wartime expansion.

Although the first effect of redeployment may appear to be the dispersal of carefully built up resources, it should lead to a better balanced Industry. Its basis is the change in emphasis between development and production. Nowadays it may take ten years before a new military concept is fully operational and its development may cost £100M. For this country at least there can be no longer any question of duplication or of alternative approaches to an operational requirement. Success must be guaranteed by consolidating the best technical experience into sufficiently strong design teams. Development must be regarded as the major factor in planning and not as a preliminary to series production. With guided weapons it is in any case difficult to draw a hard and fast line between development and production.

Fighter v. Bomber

Since its inception, air power has been conceived primarily in terms of strategic bomber forces on the one hand and defensive fighter systems on the other; and the main stimulus to aeronautical progress has been the struggle for superiority between the bomber and the fighter. Developments in propulsion, in constructional materials and methods, and in aerodynamic design have made possible progressive improvement in the performance of both, but this has not been achieved without penalty in other directions. Operational demands have been met only by accepting increasing specialisation and by sacrificing the intrinsic flexibility of the manned aircraft.

Until the end of the Second World War military aircraft were still essentially transport aircraft. The bomber had to deliver a useful quantity of military freight in the form of H.E. bombs and the fighter had to maintain a useful endurance. Improvements in combat performance had always to be reconciled with these logistic factors. Civil aircraft were, at the same time, also striving for improvements in performance. As Masefield has pointed out, there is a continuous economic demand for greater speeds on civil routes, since costs depend on hours whereas revenue depends on distance. Thus the military and civil concepts remained compatible. The military interest in speed and height as a means of eluding defences satisfied the civil interest in faster and more economic transport. In particular the aircraft engine, although sustained primarily by military demands, was readily adaptable to civil requirements.

Since the nuclear weapon became available the military requirement is for annihilation rather than attrition of the target. The strategic bomber has no longer to justify itself by making a large number of sorties; its main purpose is to guarantee the delivery of a single nuclear weapon. Performance over the target as the main criterion has encouraged highly specialised

designs which, in turn, have forced the close defence fighter to strive for equally extreme performance. Requirements have become so rigorous that design has been driven close to the asymptotic limit. Supersonic bombers and fighters are excessively large and expensive and have so little latitude in performance that they are more like projectiles than aircraft.

If these trends had continued, they would have led to an increasing diversion of resources from civil needs. In particular the engines required for such extreme military performance are unsuitable for economic transport. It is fortunate that the guided weapon is capable of meeting these demands more effectively than the manned aircraft. At supersonic speeds there is little opportunity for human intervention once a sortie has been committed. Interception by the fighter and precise attack of its target by the bomber have to be carried out automatically. The point has been reached where the crew have neither the need nor the opportunity to make any contribution and have become an embarrassment to the designer and an encumbrance during operations. The logical outcome is the replacement of the manned aircraft by the unmanned expendable missile as the primary means of imposing the deterrent and as the primary weapon of air defence.

If it had not been for the imminence of the intercontinental ballistic missile as a practicable weapon, a series of developments in the winged bomber would have been necessary to maintain the strategic threat against improving defences. The transonic bomber flying at heights in the region of 50,000 ft. already has a good chance of avoiding interception by fighters and against short range guided weapon defences its operational life can be extended by means of the powered bomb. The next phase of the winged threat is the supersonic manned bomber flying at two or three times the speed of sound and at heights up to 70,000 ft. This would severely tax even a sophisticated guided weapon defence. Closing speeds are so great that there is little time available for interception and at these heights it is becoming difficult to provide a guided weapon with enough manoeuvrability. Eventually unmanned aircraft flying at three or four times the speed of sound and at heights up to 100,000 ft. would present a problem to any form of defence. These successive developments would have been a heavy commitment for many years ahead. Moreover, they involve vulnerable ground facilities which have to be defended. The ballistic missile is cheaper and more effective and there seems little justification for continued refinement of the winged threat.

Air defence has absorbed a very large technical effort during the past 10 years. The attritional defences of the Second World War were clearly inadequate against a nuclear threat; the aim had now to be the complete destruction of every raid. The effectiveness of air defence is determined by its radar environment rather than the actual weapons available. It is the radar environment which restricts air defence to horizon range and which limits its ability to withstand large scale attack. There is no expendable air space in Western Europe and no opportunity for extending the coverage of the radar environment beyond the horizon. Thus, although the speed of both offensive and defensive weapons has increased, the time available to the defence has become progressively less and the demands on the close defence fighter increasingly severe. There is barely time to complete a pursuit-course interception even against a transonic bomber. Collision-course attack is only feasible under fully automatic control and, when the threat becomes supersonic, could prove an impracticable manoeuvre. It is inevitable, therefore, that the ground-to-air guided weapon must take over the task of air defence. It is no more dependent on its radar environment than the collision-course fighter and it is superior in performance, lethality, and economy of operation.

Thus, during the next decade the strategic bomber will be replaced by the ballistic missile and the close defence fighter by the surface-to-air guided weapon. This will relieve the aircraft designer of an onerous responsibility and allow him to reconsider aspects of design which have so far been excluded by the overriding requirement for combat performance at any cost.

The Guided Weapon

One of the earliest of guided weapons was the unmanned bomber flying to its target by dead-reckoning under the control of an auto-pilot. The British Larynx and the German V.1 were of this type. They were inherently inaccurate, subject to gross errors due to wind. The V.1 could, for instance, only be used at short range against large area targets and as the defences became effective, it proved to be an uneconomic method of bombardment.

With the discovery of radar and improvements in telecommunications, more sophisticated weapons became feasible. During the Second World War the Germans developed a range of weapons directed towards improving the accuracy of bombing, particularly against small but easily recognised targets such as ships; to extending the height coverage of anti-aircraft fire; and to increasing the lethality of the fighter. These early designs were dependent on visual or crude radar observation of the target and on subsequent command guidance. They were simple open loop systems limited in range and accuracy, susceptible to errors in prediction, and liable to be ineffective against a manoeuvring target.

It was only after the Second World War with the development of lock-follow radar that really accurate target information became possible. This allowed error signals to be fed back into the guidance system to give continuous closed loop control. The full potential of the guided weapon could now be exploited with consequent improvements in accuracy and range. The most sophisticated form of closed loop control is the homing weapon which contains within itself all the sensing and responding apparatus required to intercept a manoeuvrable target; and the homing ground-to-air weapon made the replacement of the fighter an achievable objective.

The guided weapon provides a simpler and more effective solution to the operational requirements of air defence. The mere absence of a crew with all their com-

plicated services and essential safeguards is a tremendous relief. Moreover, since the weapon has only to make the outward journey, it is a more efficient method of delivery. It is not surprising that surface-to-air guided weapons reduce the cost of close air defence. Costing depends on a number of assumptions, such as the scale and duration of the attack, the area to be defended, and whether or not there is an existing infrastructure, but a guided weapon defence should be several times cheaper than a fighter defence, even against the existing threat.

The guided weapon has, however, a much greater potential performance than the fighter. Its thrust to weight ratio is greater and it is able to withstand more severe operating conditions. As a result it climbs more rapidly, reaches a higher ceiling and is much faster. Its smaller size and stronger construction allow it to develop greater lateral acceleration and to respond more quickly to control. It can deal more effectively with target manoeuvre during interception and with aiming errors in the final phase of the attack; and it carries a much larger warhead. The surface-to-air guided weapon should match the winged threat for a long time to come.

During the past ten years we have gained invaluable experience in the design of these weapons and now have a well established industry capable of developing and producing a range of advanced weapons. The new teams drew heavily on the knowledge and resources available in the electronics and aeronautical industries and the resultant cross-fertilisation has been of value to both. The merging together of system engineering, which has always been a necessary discipline in electronics, with the meticulous design standards of the aeronautical engineer has been particularly fruitful.

Aerodynamically the expendable weapon is much simpler than the manned aircraft. It is concerned with a narrower range of flying conditions since it has no landing problem and at take-off is boosted almost immediately to its supersonic cruising speed. The aerodynamic simplicity is, however, more than countered by the complexity of the system as a whole. It is control and guidance which restrict the weapon rather than aeronautical techniques. These alone would permit far more impressive performance, particularly in range, than can usefully be exploited under warlike conditions.

The guided weapon is dominated by the final phase of interception almost as completely as is the manned aircraft by landing and take-off. Its sole function is to bring a warhead into sufficiently close proximity to a target. This can only be achieved by a completely integrated and tightly coupled closed loop involving three essential functions. The guidance, usually radar, must acquire the target and continuously measure any deviation from the required collision course; the control system must translate the error signals into suitable commands and the missile itself must develop high lateral accelerations in both planes in order to correct the errors with sufficient rapidity.

Aircraft design has, of course, always involved compromise between competing requirements but the components are more loosely coupled and can be optimised separately to a greater extent than is tolerable with the guided weapon. Development can depend on a long series of flight trials involving up to 1,000 hours of experimental flying. During these trials problems can be isolated and dealt with separately; and the final design can be refined by successive improvement. This is out of the question for an expandable weapon; the cost alone would be prohibitive. Each firing should be the final confirmation of a lengthy programme of test and simulation on the ground. Even so, many hundreds of rounds must be fired during development to establish the reliability of the complete weapon for Service use.

These trials which, although they extend over many years, involve a total flying time of less than an hour, are controlled by the most elaborate simulation on the ground. The simulator is the key to guided weapon development. Initially the behaviour of the system is explored and the concept established largely on a theoretical basis; but as trials proceed, simulation is refined by incorporating actual components until eventually the action of the complete system under a wide range of conditions can be accurately reproduced. Only in this way is it possible to analyse the success and failures of a complex weapon during the later stages of development.

The Ballistic Missile

In contrast to defensive guided weapons, the outstanding characteristic of the ballistic missile is its independence of the external environment. Control and guidance can be entirely self-contained and, after the initial launching phase, the missile continues on an invariant ballistic trajectory until it re-enters the atmosphere.

The missiles now becoming available are single-stage weapons using a bi-fuel liquid motor and directly derived from the German V.2. It may be thought somewhat remarkable that 15 years have elapsed since the Germans first demonstrated the military possibilities of the ballistic missile and yet they are only just coming into service and have still to establish their reliability. However, the V.2, although a tremendous technical *tour de force* at the time, was of elementary design by present standards. It weighed about 12 tons and delivered about one ton of high explosive over distances not exceeding 200 miles with an accuracy of about 5 miles. There was every inducement for the Germans to extract an immediate military return from its limited performance. They had lost control of the air and there were a number of profitable area targets, notably of course London, within range. The Allies on the other hand, were entirely free to use the superior coverage, capacity and economy of their air forces. The potential advantages of the ballistic weapon were obvious but it was not until the lightweight megaton warhead became available that they could be properly exploited.

Current weapons have at least ten times the range and ten times the accuracy of the V.2, their thrust is of the order of 100 tons and the structure weight is of the order of 3 per cent of all-up weight. Design of large rocket motors is still at an early stage and we must

expect a period of technical improvement directed towards higher specific impulse, better control and, above all, greater reliability. Specific impulse is an even more critical parameter for the rocket motor than it is for aircraft engines, since the range of ballistic missiles depends on the square of the specific impulse instead of directly, as for aircraft. Control of thrust is also desirable since it allows excessive accelerations to be avoided with a consequent easement in the design of the structure and of the delicate control components; and it permits the maximum range to be extracted from a given quantity of fuel and oxidant. Improvements in reliability and in readiness are necessary before the ballistic missile can be regarded as an entirely dependable weapon. It is in these respects that solid propellants offer promise for the future.

Structure weight is, as always, a critical design factor but here again the effect on range is more severe than for aircraft and the requirements correspondingly more stringent. The ratio of structure weight to all-up-weight of the missile is an order of magnitude less than for aircraft. This calls for a new approach involving, for instance, pressurisation as a structural method.

However, by far the biggest challenge lies in the accuracy demanded of the guidance system, which moreover, must be maintained under extreme conditions of vibration and acceleration. The performance required of the inertia components bring us to the very limit of instrument techniques. As an indication of the refinement of design, a deviation of the centre of gravity of a gyroscope by only a few wavelengths of light can lead to an error of several miles at the end of the trajectory.

Finally, there is the problem of re-entry. A long range ballistic missile or a satellite must dissipate a large amount of energy as it plunges into the atmosphere. Most of this is left behind in the wake but the small proportion left over is still very large and can heat up the missile to very high temperatures. As an example, a spherical satellite of about 1,000 lb. in weight and a specific gravity of 3 reaches, during re-entry, a temperature of 2,000° absolute and a luminosity comparable with full moon. Very special measures are needed to ensure that equipment survives the high temperature and high accelerations arising during re-entry.

As is so often the case, all these problems which at first appeared so formidable, have turned out to be solvable and inter-continental ballistic missiles of sufficient range and accuracy are clearly on the verge of achievement. The operational value of the ballistic weapon lies in its superiority over foreseeable defences, and in its relative invulnerability to counter-attack. Its superiority over the defence is likely to remain a dominating factor for at least the next decade and this alone will greatly reduce the cost of maintaining the deterrent. The second advantage of the ballistic missile is that the entire launching site can be underground, protected from anything bar a direct hit with a nuclear weapon. Initially security will lie in the difficulty of identifying and precisely locating these bases; but even when they become known, the accuracy of guidance, while adequate for attacking areas such as cities and even aerodromes, is unlikely to guarantee a direct hit on a buried launching site. When the deterrent is based on the ballistic missile rather than aircraft, its defence will involve much smaller resources than at present.

American progress in the military field and the impact of the Russian Sputniks has stimulated public interest in the possibilities of space travel. It will be a long time before we can venture into outer space with a reasonable chance of safe return and we may have to wait for new forms of energy before the wilder conjectures of science fiction become realisable; but even with chemical fuels, some of the next steps can be identified with reasonable confidence.

Kerosine and liquid oxygen, which are typical of present liquid propellants, burn at about 3,400° absolute and the exit velocity of the gases is about 1½ miles/sec. If 90 per cent of the weight is fuel, and less than 10 per cent is structure and payload, the final velocity can be built up to about twice the gas velocity, *i.e.* 3 miles/sec. This will allow a missile, if fired straight up, to reach a height of about 1,000 miles above the earth, or if fired on a optimum trajectory to achieve a range of about 2,000 miles.

Greater velocities can be built up by using multi-stage rockets. A three-stage missile, for instance, allows 99 per cent of the initial mass to be expended in building up the momentum of the remaining one per cent; in this way the velocity of 5 miles/sec. required for a satellite can be achieved. To escape altogether from the earth's gravitational field requires a velocity of 7 miles/sec. and with present specific impulses and structural methods would demand a take-off weight at least a thousand times the final weight.

In terms of the high grade technical effort involved these are very large undertakings and may well prove to be beyond the resources of any one country. However, we should not feel too intimidated by the task of launching such huge missiles. The chemical energy dissipated as high explosive in one bomber raid during the last war is not a high price to establish a satellite in an orbit for several years, or to land a beacon on the moon. Such ventures would undoubtedly stimulate technical progress throughout industry, but they must be justified by the amount of new knowledge acquired. We must first solve the problems of elaborate telemetry over a long period and of controlled recovery. Both problems are certainly solvable with the resources available in the electronic and aeronautical industries. There is no doubt that mankind is on the threshold of a fascinating new adventure; the extent to which this country can afford to contribute is another matter.

The Manned Aircraft

Although the manned aircraft is being superseded by the guided weapon in two of its traditional roles, it does not follow that it no longer makes a vital contribution to air power. On the contrary, it is still the only means of providing transport, surveillance and tactical support, and the requirements of peripheral war are likely to increase, rather than decrease, the importance of these roles.

Surface-to-air guided weapons will no doubt be used for the defence of overseas bases and ballistic missiles

could be effective against fixed targets in rearward areas. But the guided weapon is useless unless it is supplied with precise target information. The strategic threat is imposed primarily on areas whose position is already disclosed and in air defence the target is discernible against an empty background. In field warfare forces will be mobile, dispersed, and concealed, and before they can be attacked they must first be discovered. Military concentrations behind the front line will have to be located by reconnaissance from the air; and the aircraft which carries out this reconnaissance could also deliver the strike. There is at present no real solution to defence against low level attack and this could become the preferred method for ensuring both tactical reconnaissance and strike.

The interceptor fighter may also have a useful function to perform. Its failure as a weapon of close defence is due to its restricted environment and to the high performance of the strategic bomber. But in peripheral warfare there may be sufficient air space to permit fighter operation against aircraft engaged on transport, reconnaissance, and maritime surveillance; and developments in airborne radar may make fighter defence feasible against the low flying threat.

Transport remains as always an important aspect of air power, but much of the advantage of transporting military forces by air can be wasted by time required to bring forces into readiness, by dependence on permanent aerodromes which are not always available close to the scene of operations and, above all, by lack of numbers. There are rarely enough aircraft to avoid a slow build-up by successive lifts. Air transport could also make an increasing contribution in the front line and may prove less expensive than is usually supposed if proper account is taken of the savings in road transport and of changes in front line requirements. A large proportion of the stores required by an army in the field is consumed by the logistic tail and significant reductions would be possible if delivery to the front line could be guaranteed. In limited war air transport need not be excessively vulnerable to interference by the enemy. Low flying, particularly at night or in thick weather, poses a difficult interception problem, especially when the sorties are well dispersed.

We need not fear that the termination of the competition between fighter and bomber will destroy the stimulus to further aeronautical progress. Apart from the new scientific and technical problems presented by guided weapons and ballistic missiles, there are many possibilities yet to be explored with manned aircraft. To mention only one, military aircraft must become less dependent on ground facilities if they are to avoid counter-attack; and various forms of jet-assisted lift could lead to new approaches to landing and take-off and to more efficient performance in flight.

Conclusion

It is never wise to be too dogmatic about the future and it is particularly dangerous where technology is rapidly developing. On more than one occasion the attention of this Society has been drawn to the apparent impossibility of progress beyond some theoretical limit only to have it proved wrong in practice shortly after. Usually the argument has been sound, but the assumptions ill-founded. I must emphasise, therefore, that the survey which I have given depends on the continued supremacy of the means of delivery and hence on the acceptance of the logic of retaliation.

Engineers are somewhat wary of general theories or of long term predictions, but in an art as complex as aeronautics there is danger, in the absence of a clear technical strategy, of dissipating effort over too wide a field. Moreover, it may be 10 years before a new project becomes operational and design must anticipate the conditions which will then prevail.

Scientific and technical developments can no longer be regarded as perturbations in an established environment. On the contrary they now govern the conditions under which we live and our whole economic future. Scientists and engineers cannot escape their responsibility for planning the allocation of their resources. I hope I have provided a background for more authoritative and detailed assessment by members of the Society of the trends in aeronautics.

DISCUSSION

Dr. G. W. H. Gardner (Director, R.A.E., *Fellow*): Dr. Cockburn was always interesting to listen to and remembering that he had been Scientific Adviser to the Air Ministry, was now the Controller of Guided Weapons and Electronics, and had always been acutely aware of the impact of scientific advance on strategy, they expected a stimulating lecture and had not been disappointed. They were grateful to him for shedding light on the profound effect of guided weapons on the aeronautical scene. He had put this in its proper perspective by pointing out the greater effecte of the emergence of nuclear warheads which had conferred on offensive power a superiority it was likely to enjoy for a long time.

Dr. Cockburn had made it clear that guided weapons would not satisfy all military needs and that manned military aircraft had important roles to perform in the future. However, he would suggest that the reduction in the number of advanced military aircraft likely to be required in the future which resulted from the power of the nuclear warhead, and the assumption of some important roles by guided weapons, were likely to have a more profound effect on their Industry than Dr. Cockburn seemed to suggest. The swing of emphasis from production to development, which Dr. Cockburn predicted, was likely to reduce incentive in industry and reduce the fruits of development in the shape of new materials and techniques becoming available to other industries. This, in the past, had been an important contribution of the progressive Aircraft Industry to the national economy. The adverse effect of the reduction in the number of aircraft required would, of course, be compensated in part by the guided weapon activity which, even in the course of development, involved small-scale production.

H. H. Gardner (Director and Chief Engineer,

Military Aircraft, Vickers-Armstrongs (Aircraft) Ltd., *Fellow*): Dr. Cockburn had made a clear case for a period of stability called, he thought, "strategic stability," due to the progress in offensive nuclear weapons without the corresponding advance in defensive systems. He welcomed this statement as it was, undoubtedly, a valuable assumption and produced an excellent starting point for future planning.

The long term development in the Aircraft Industry, following the White Paper on Defence, had been and continued to be a problem of major importance in the Industry. Any statement which would clarify and help in the long term planning, both of technical strength and production capacity, was of the utmost value.

Dr. Cockburn stated that although he had heard of alarm and despondency in the Aircraft Industry, as yet he had not personally seen this. If this were so, Dr. Cockburn must have avoided all but a very small section of the Aircraft Industry, as those who were very closely involved, particularly with the Military side, were working at the present time in areas of great depression, real despondency and staff problems which now contained an element of tragedy. He would suggest that Dr. Cockburn should extend his contact with the Industry, as he was sure he would find reason to change his views radically.

In the paper it was stated that the mere absence of a crew with all their complicated services and essential safeguards was a tremendous relief. This might be true for a simple missile, but there was another aspect to this problem. In a complicated vehicle, such as a modern fighter, the attendant problems of eliminating the crew or reducing their functions to that of monitoring had, in fact, brought about a surprising complication in the equipment of the aircraft. Although it was undoubtedly true in simple weapons that the absence of a crew reduced the size of the problem, it was also true that, in the more advanced aircraft, the absence of a crew introduced heavy complications and, in the development period, created a problem which could become financially impossible.

He was interested to follow Dr. Cockburn's argument that all Military Aircraft were essentially Transport Aircraft, particularly up to the end of the last World War. He would suggest this was a debatable assumption as he had been particularly impressed in the development of fighter aircraft, for example the Spitfire with its ancestry of the record-breaking seaplane, the Schneider Trophy winner, the S.6, which would need some stretch of imagination to be classified as a Transport Aircraft.

He would welcome Dr. Cockburn's views on two particular future developments. In view of the development of the Intercontinental Ballistic Missile and the position of stalemate which might well be reached—also having in mind its cost and relative vulnerability at launch—he would be interested to know how far the Navy would be involved in accepting the offensive role in any future combat, particularly the nuclear-powered submarine armed with the medium range missile, and the carrier-borne strike aircraft armed with a small atomic bomb. How far security would allow a discussion of the Navy's role in these two instances he could only guess but, within these limits, he would be extremely interested to learn how far the arguments propounded in Dr. Cockburn's excellent lecture led to an increase in these two activities associated with the Senior Service.

Dr. R. R. Jamison (Assistant Chief Engineer, Ram-Jets, Bristol Aero-Engines Ltd., *Fellow*): Dr. Cockburn had given a most lucid account of the military position in the air as he saw it and had provided an authoritative framework for some very hard thinking by all aeronautical engineers in this country. Nonetheless, certain bold assumptions were made on which the rest of the arguments were constructed. Those assumptions embraced not only the technical features in design and manufacture of military equipment, but also psychological ones involving assumptions as to what people would do in given hypothetical circumstances and, moreover, the structure of logic of the deterrent was built on the assumption of a conflict between only two contestants. He believed that the picture might be very different, with three, four or many powers coming to hold the deterrent and this multiplication of possible deterrent holding combatants appeared likely to occur in a relatively short time, so that the validity of the set up for a two-sided conflict might be of short duration. In particular, as soon as more than two combatants were assumed a very real problem would arise if a nuclear explosion should take place as to "who threw it?" before the inevitable retaliation could be applied.

There was another picture coming into being reinforcing this query. It was clear that in addition to nominally invulnerable land bases for launching missiles, they might also be launched from long-range submarines or aircraft with long range powered bombs operating from any part of the world. He would be interested to know if Dr. Cockburn considered these features would affect the neat picture of blow, counter blow, which he had drawn for them.

He would now like to comment on the likely effect of the new weapon philosophy on the Industry. If development became dominant and production became a secondary activity, then it followed that in a commercial industry the development must be made worthwhile. No longer could Industry look to production to repay capital expenditure. Speculative essays into weapon design became most unlikely because the quantity was likely to be extremely small compared with the risks and costs involved.

Dr. Cockburn said that there could no longer be duplication or alternative proposals to meet operational requirements. This might be so, but the so-called duplication was not necessarily a wholly bad system. Rightly administered it could produce valuable results. If the customer, *i.e.* the armed forces and their agents in the Ministry of Supply, were not certain what would be the best solution to the military need, he could set a number of teams to work on investigations. In the normal course of events this would produce a series of allied but different lines by teams of intrinsically different capabilities. This was not simply duplication, and some of the work done in this way, if properly co-ordinated, became the basis for further advances.

On the other hand, if they took literally Dr. Cock-

burn's proposal to consolidate the best experience into a sufficiently strong design team, they would require a superior authority capable of choosing such a team and deploying it effectively. This would imply the highest standards of engineering, scientific and organisational competence in the co-ordinating authority and he respectfully suggested that Government agencies would need as drastic a redeployment as was implied in industry.

It was now becoming evident that the streams of development for military and commercial purposes in aeronautics were diverging so that civil aircraft would no longer be able to look entirely to military prototypes for the necessary technical advances, and they were told that the Government considered that commercial aviation should be much more self-supporting. However, it appeared to him that if this were brought to pass the rate of advance in civil aviation would be slowed down to a progress comparable to that which they had seen in merchant shipping. In fact, little significant advance in ship design had been evident over the past thirty years. This might not matter if foreign competitors followed a similar policy, but at the present time foreign countries were still continuing extensive military aircraft programmes to the benefit of their civil machines which would place this Country at a tremendous disadvantage.

The lecture had been an authoritative assessment of the influence of guided weapons on aeronautics and he thought that they were fortunate to have a lecture of this calibre for the newly-formed Guided Flight Section of the Society.* The proposal to found this Section was put to the G.W. Technical Committee of the S.B.A.C. and, at the time, he opposed it as he thought that adequate scope for these topics was provided within the ordinary framework of the R.Ae.S. However, Harry Gardner met his opposition by getting him on to the Guided Flight Committee and since this Section had been active he had been fully converted as to its value to the Society. Under its auspices they had had a series of important and valuable lectures.

E. T. Jones (Deputy Controller (Overseas Affairs), Ministry of Supply, *Fellow*): Seldom, if ever, had a successful weapon of war been conceived, developed to its peak, and replaced by a different kind of weapon in one life-time. Nevertheless, it would seem from what they had heard that this was precisely what was likely to happen to the most successful weapon of war the world had ever seen—the manned military aircraft.

Knowing the profound thinking that Dr. Cockburn had given over many years to this subject he would be one of the last to disagree with his arguments or with his conclusions. Indeed as Dr. Gardner had already said, there was probably no one better able to explain so explicitly as Dr. Cockburn had, the reasons why there was no need at present to develop the manned military fighter and bomber much further.

However, to meet the demand for speed, range and accuracy the arrow of "1066 and All That" gave way to the projectile and the projectile gave way in due course to the manned aircraft and now they had the process in reverse; the biplane shed a wing as the lecturer had said and the manned bomber was now shedding the remaining wing and becoming again a projectile. He wondered how long it would take before they arrived back to the arrow. They would then at least get large production orders again, the current lack of which had been lamented by the two Gardners.

There was one argument in Dr. Cockburn's lecture which he found some difficulty in understanding. Dr. Cockburn had said that, as the threat became reciprocal, nuclear weapons provided the basis for a truly stable military environment. He did not quite see why Dr. Cockburn felt that the nuclear weapon in-so-far as stability between communities was concerned was different from the revolver in-so-far as the stability between individuals was concerned. Two individual enemies each with a revolver constituted a reciprocal threat, but the revolver had not led to stability between individuals. Why, therefore, did Dr. Cockburn expect or hope that stability between communities would become real through ownership of a nuclear weapon? He would have thought the only possibility of this happening was to have the revolver so made that it killed the man who pulled the trigger simultaneously with the man who received the bullet, and that both knew that this would happen if the trigger be pulled.

S. Scott Hall (Scientific Adviser to the Air Ministry, *Fellow*): Dr. Cockburn's paper would become a classic and he felt that the Society was greatly indebted to him for such a clear statement of the present position of guided weapons and their application to defence.

Dr. Cockburn was always thinking ahead, and if he might make a general criticism of the paper, it was that perhaps the timing of events as stated by Dr. Cockburn was optimistic. He would suggest that in some sentences the word "is" should be replaced by "will be."

He also felt it was a pity that more quantitative support could not have been given to back the author's arguments, but perhaps security prevented this.

He felt that there was one condition applying to the stalemate of mutual deterrence produced by the nuclear weapon, which had not been stated, and which was essential. This was that the launching arrangements for the nuclear weapon should be invulnerable.

The point made by Dr. Cockburn that the logistic basis of war was changing and that military strength would depend on resources built up in peace-time was exceedingly important and could not be over-emphasised.

He wondered why Dr. Cockburn had stated that the surface-to-air guided missile could carry a much larger warhead than a fighter. He did not think there was any fundamental reason for this.

Finally, the concept of the deterrent must depend on the enemy being fully aware of how effective the retaliatory force was. This seemed to him to pose a most peculiar problem for the intelligence people, of a type which they had never had before.

R. A. Shaw (Assistant Director, Aircraft Research and Development, Ministry of Supply, *Fellow*): It was admitted that they needed guided weapons and aircraft.

**Note*:—Although of interest to the Guided Flight Section, Dr. Cockburn's Lecture was a Main Lecture.—ED.

With limited money they had to concentrate on the more important things, which were the guided weapons because they provided *the* deterrent. But they had been told that they would soon reach their peak of development, they would be perfect. When this was so they would presumably stand about, shining in their glass cases, serviced occasionally but, they hoped, never used. Never using them, they would not have to make any more. The available funds would flow again to manned aircraft which brave young men would fly about the world, wearing them out and occasionally breaking them. This was the golden age aircraft manufacturers were waiting for. How long must they wait?

His second point was serious. They had been told that the guided weapon with a thermonuclear warhead was *the* deterrent. They had also been told that there was no objection to more nations possessing this weapon. He saw a danger there. Suppose Nation A and Nation B had a dispute, the argument was hot, but there was no fighting yet. Nation C, which liked neither A nor B, lobbed a "deterrent" weapon into the capital of one of them from a submarine off-shore. If war then flared up between A and B, their "deterrent" weapons would have been no protection. They had been told "nuclear weapons provide the basis for a truly stable military environment." When more than two nations possessed them, he doubted if that were true.

N. E. Rowe (Technical Director, Blackburn and General Aircraft Ltd., *Fellow*): Dr. George Gardner had referred to the great value to engineering as a whole of the pioneering work done in aviation, which had continually led to the development of new materials, new techniques, new equipment of various kinds and new standards of manufacture, which in turn had filtered into general engineering to its great benefit. Did Dr. Cockburn think that the development of Guided Weapons was likely to have the same kind of influence.

He thought that the very clear and courageous paper which had been given to them would form a landmark in the affairs of The Royal Aeronautical Society of this country, and that this would go down into their archives as one of the classic papers which at critical periods in the past had shown the way into the future.

Group Captain Charles F. Sarsby (R.A.F. (retd.), Ministry of Supply Engineer, *Associate Fellow*) *contributed*: Dr. Cockburn referred to aircraft as a method of transport but later he allowed other interpretations.

In the R.A.F. it was taught that they were a fighting service and that the aircraft was their means of transport. The weapons of war were the bombs, guns and ammunition, which they carried. He believed it to be an important doctrine because, although, as Dr. Cockburn explained, the atomic warhead was a revolutionary weapon and its use would obviate the necessity for bombers and fighters as they had known them, it did not alter the fact that the aircraft was the fastest and most economical means of transport. As such it would have a continuing and permanent use in the fighting services.

He wrote in the hope that they would soon see the development of a different type of aircraft—above all—one so easily manufactured in quantity that it was uneconomical to expend guided weapons on its destruction.

DR. COCKBURN

Dr. G. W. H. Gardner raised the question of redeployment from production to development with which the Aircraft Industry was faced. There was no doubt that this was disturbing for an industry which had for many years been organised for fairly large scale military production. However, he hoped Dr. Gardner was not right that this would destroy incentive; and that instead the Industry would discover itself to be in a stronger competitive position. The trend towards reduced numbers of aircraft was not confined to the military field. Civil aircraft were becoming larger and faster; and fewer would be needed for a given passenger and freight loading.

Mr. H. H. Gardner raised the question of the Navy's role in future air warfare but would not expect him to go into too much detail on this subject. However, Naval air power clearly had an important peripheral role. Aircraft carriers with all their built-in services afforded a flexible means of providing, at short notice, tactical air support on a limited scale. It was too early to say what contribution the Navy should make to the strategic deterrent. An essential factor underlying the concept of nuclear stalemate was the demonstrated ability of the deterrent to survive a counter attack. This might be achieved either by relatively invulnerable buried bases, or by mobility and dispersion. If the latter alternative should prove preferable, the Navy would have an important additional role to play.

He agreed that the transport aspect of military aircraft must not be pushed too far, but even in the fighter field the loosely controlled night fighter must have a much greater endurance than was necessary or possible in the closely controlled collision course fighter.

Both **Dr. Jamison** and **Mr. Shaw** wondered whether stability would still exist if several powers possessed a nuclear threat. He thought the thesis remained valid. Retaliation imposed the same restraints on all nations. The deterrent was a defensive instrument which displaced Fighter Commands rather than Bomber Commands!

Dr. Jamison was perhaps envisaging an irresponsible madman in control. They could never completely rule out such a possibility, but they should not plan for catastrophe. It would be the political concern of all Governments to prevent such a situation developing.

Dr. Jamison made a pertinent point, that absence of all duplication might also remove competition in ideas. They could not afford a policy of duplication, but too much authority could only lead to technical sterility. The situation must be watched carefully.

Mr. E. T. Jones's analogy between the nuclear stalemate and the two duellists was not quite complete. In addition, each duellist must realise that his opponent's second had got him covered. The backward firing gun, although a horrifying idea, was a still closer analogy.

Mr. Scott Hall would appreciate that he had avoided any reference to timescale. However, although the factors he had been discussing might only mature during the next decade, they must already affect decisions.

FRANCIS HERBERT WENHAM, *Honorary Member* 1824-1908

An Appreciation of the First Lecturer to the Aeronautical Society

by

J. LAURENCE PRITCHARD, *Honorary Fellow*

(The following is an extract from Chapter 5 of the MSS of the History of the Aeronautical Society, now being written by Captain Pritchard:—

"As the Society at the time this is being written is only about a decade from celebrating its hundredth birthday, and so a better assessment can be made of the lasting value of the ideas and work of early aeronautical pioneers. it is of interest to quote D. S. Brown's opening words in his lecture on The Aeroplane—Construction, Stability and Means of Propulsion, read on 30th June 1873.

"'It is more than half a century since Sir George Cayley published the results of his researches in elucidation of the problem of Aerial Navigation, which was followed several years afterwards by the celebrated project of Mr. Henson, and since then has also appeared a valuable contribution by Mr. Wenham.'

"Cayley, Henson, Wenham, three names which can never be left out of any history of heavier-than-air flight, three names which occur regularly in the lectures and discussions of the Society in the first thirty years, and regularly, too, in the letters which enthusiasts wrote to explain their own ideas, for they always wanted something a little more solid than their own ideas to give them confidence."

Papers on Cayley and Henson have appeared in the JOURNAL. *This paper completes the trilogy named by Mr. D. S. Brown 85 years ago. The greater part of the information in it is new to the Journal of the Society.—Ed.)*

"Of this we may be sure, in years to come, when the early history of aviation has become veiled with the dim twilight of half-forgotten things, the figure of Wenham will stand out clear and strong above the deepening shadows."

Aeronautical Classics, No. 2.

Edited by T. O'Brien Hubbard and J. H. Ledeboer, London, 1910.

Introduction

In December 1905 Wilbur Wright wrote to Octave Chanute, "We were indeed very much gratified at the commendation of Mr. Wenham, whom we regard as one of the ablest and most useful men who have laboured in the cause of human flight."

Wenham had written to Chanute expressing his delight at the success of the two brothers and adding, "It removes me from the list of cranks who always stated I was advocating an impossibility."

He had been so listed for forty years.

Chanute himself held Wenham in such high esteem that when the Wright Brothers, in November 1905, gave him permission to reveal some of the details of their flights and experiments, Chanute replied, "I shall avail myself of your leave to keep no longer secret the results of your experiments by advising Wenham confidentially, but will not volunteer information to others."

It was due to Wenham that Chanute himself took the interest he did in the possibility of flight, and in his letter of 5th November he wrote, "I hope all this will enable you to exult to yourself without telling anybody."

Wenham died on 11th August 1908, in his 85th year and a Memoir was published in *The Aeronautical Journal* of the Society in October 1908. It is reprinted in *full* on page 572.

This may justly be described as the aeronautical Memoir to end all Memoirs. It does not even give the dates of the birth or death of the man whom the author of the Memoir was prepared to crown as the father of aeronautics in Great Britain, and the author of one of the "most important papers ever published on aeronautical science." Nor did the Council of the Aeronautical Society make any reference in their Minutes of the loss of their most distinguished member.

I hope, in this present paper, to show that for his variety of interests, his impact upon his contemporaries, and for his clarity and constancy of his advocacy of the coming of the aeroplane, Wenham may be compared

THE LATE MR. F. H. WENHAM, HON. MEMBER OF THE AERONAUTICAL SOCIETY OF GREAT BRITAIN

Members of the Society will hear with great regret of the death of Mr. F. H. Wenham, one of its Hon. Members. Mr. Wenham was one of the leading members of the Society at its foundation, and may almost be called the "father" of aeronautics in this country. His paper on "Aerial Navigation (1866)*" is one of the most important ever published on aeronautical science, and contains a great deal of valuable information.*

It should be specially noticed that he was the first to advocate the superposed planes now used by Farman, the Messrs. Wright, and others, and there can be no question that his ideas of reducing the great width required for support by placing narrow supporting surfaces one above the other is one that has been most helpful to aviators.

Mr. Wenham always took a great interest in the Society, though his health of late years prevented him doing much active aeronautical work. The paper by him, published below, appears to be almost, if not quite his last contribution to Aeronautical Science.

From "The Aeronautical Journal," October 1908. The second paragraph of this Memoir is incorrect. Sir George Cayley suggested biplanes or triplanes to lessen too great a span.

with Sir George Cayley, whose work he acknowledged freely at the first meeting of the Society in 1866.

In the annals of aeronautics appear the names of many who have made important contributions to its advancement. Many have lived a more varied life of achievement than is usually given in the memoirs which are printed in the technical Journals of the Societies to which they have brought lustre.

I believe that it is of some importance that attention should be drawn to their activities in other fields, in an age when specialisation is leading to many unbalanced views of progress.

Early Days

Wenham was born in Kensington, London, in 1824, the son of an Army surgeon. In those days Kensington was very much of an agricultural district, sending considerable quantities of hay and vegetables to the London markets. At Kensington Palace a child princess was being educated. She became the great Queen Victoria, whom Wenham survived by seven years.

Wenham was a boy of nine when the large red brick mansion, Rutland House, opposite Knightsbridge Barracks (built about forty years before) was pulled down and the grounds sold for building. Here, in the roadway nearly opposite the western end of Rutland Gate once stood the notorious old Inn known as the Halfway House, with an array of stabling, troughs, pigsties and so on. At one time it was the resort of highwaymen and footpads.

Wenham, as a boy, had revealed a keen mechanical mind and early showed the temperament and questioning interest of the applied scientist and practical engineer.

In his seventeenth year Wenham was articled as a pupil to a marine engineering firm in Bristol. Only a year or so before, the *Great Western*, a paddle steamer designed by Brunel, left Bristol on 8th April 1838, for New York, the first steamship to do so. The Great Western Railway built the steamer as a natural extension of their passenger carrying work, and it proved so successful that it was decided to build a still larger vessel, the *Great Britain*, to be fitted with a screw propeller.

"My training has been that of a marine engineer," wrote Wenham, in the last article he ever wrote, published in the same issue of the *Aeronautical Journal* announcing his death. "I was present at the early hours of *Archimedes*, the only screw steamer afloat. I was but a small boy then, but ever since have taken a keen interest in screw propulsion."

As a pupil in the drawing office at Bristol his interest was aroused by the constant discussions going on about the "new-fangled" screw and the well-tried paddle-wheel as methods of propulsion of water craft. Soon the young Wenham knew all the pros and cons of both. He came down on the side of the screw and was convinced it would entirely supersede the paddlewheel. It was many years before it actually did so and, in fact, it was not until 1862 that the Cunard Company obtained permission to use screws in their mail boats.

"The trials with the *Archimedes*," wrote Wenham some years later, "led to the construction of a small river steam boat of shallow draught propelled by four screws; these were driven by leather belts, which worked smoothly. Since then I have constructed and experimented with various forms of screw propellers for marine and aerial purposes for which the two-bladed form is most efficient."

Engineering Emprise

Wenham's first independent work, after he had finished his training, was on the design of a high pressure steam tubular boiler for marine engines. The normal pressure at that time was round about 60 lb./in.^{2}, but Wenham boldly carried out experiments at five times that normal, producing an engine which worked at 300 lb./in.^{2}. Preliminary tests of the new boiler convinced him that it would prove more economical and reliable than those then in use. He decided to try it out in actual practice.

For that purpose he designed a small steam vessel and built it with the aid of two mechanics. Tests were made on the quiet waters of the Thames, and proved very successful.

A few years before Wenham had begun his marine engine experiments, Daguerre had published in France, in 1839, a description of his photographic process. Wenham immediately took a great interest in the new idea, and in the improvements which were being made by others. He had become acquainted with F. Frith, an enthusiast who became the founder of the well-known publishing house of famous pictures.

Frith suggested to Wenham that he should come out to Egypt with his new little steam boat and take a trip up the Nile, so that Frith could take photographs. Wenham was delighted, because it gave him an unusual opportunity for testing thoroughly his new engine and ascertaining its full capabilities.

Wenham's boat was too small to risk sailing across the Bay of Biscay and so it was shipped to Alexandria.

During the journey up the Nile, as far as the second cataract, Wenham proved of considerable help to Frith, who was not only anxious to photograph scenery and famous places of interest, but also the interiors of some of the Egyptian tombs. Wenham devised a series of mirrors for reflecting the sunlight into the darkest corners of the tombs, and it was due to Wenham's lighting ideas that the inscriptions on the walls could be clearly photographed and read.

The photographic tour up the Nile was so successful that on the return to Alexandria it was decided to sell the steamer and to pay a visit overland to the Holy Land, by way of Mount Sinai and through Jerusalem and Damascus to Beirut, names far more familiar to most people at the present time than they were then. The series of photographs which was taken had a very wide sale, and was the first indication of the coming of the picture postcard.

Wenham was a good shot and added to his usefulness by providing a change of food for himself and his companions. He did not fail to make use of his skill and draw conclusions about bird flight from those birds which escaped his gun, as well as those which fell victims to it. The conclusions which he drew on the Nile were to prove invaluable material for his paper of 1866 to the newly-fledged Aeronautical Society.

It is of interest to note that Wenham's first patent, taken out on 1st February 1854, was for a special device to prevent a sporting gun being fired accidentally.

The success of the high-pressure boiler of the steam engine induced Wenham to enter into a partnership to build a gas engine which he had designed, and which he believed was the first of its kind. During the next few years he put forward a number of patents for improvements in steam engines.

At the end of 1859 Wenham, in his patent No. 2695, put forward his invention "chiefly applicable to marine condensing engines worked by the expansion of high pressure steam."

"Steam of a high pressure," to quote his patent, "is led direct from the boiler to the non-condensing engine; after having been there worked expansively it is returned into a series of pipes placed in the chimney, known as a "superheater"; being thus charged with heat, it is next carried back to the cylinders of the condensing engine, expanded again, and exhausted into the condenser . . . ."

He followed this patent with a further one on the same subject in 1860, in which he stated that his claim was "intermediate application of superheating steam in its progressive expansion between two or three cylinders . . . ."

The development of the use of superheated steam meant a considerable increase in the efficiency of the marine steam engines in use. By 1861 the American Civil War had broken out and superheated steam gave an increased speed to the ships plying between the Clyde and South America, speed which was to prove a great advantage to those using it, to run the blockade.

But Wenham was threatened with a law action on the grounds that a similar use of superheated steam had been proposed some years before. Although he was in business as an engineer, he would never carry any dispute about his patents into the law courts. He often saw how a patent in existence could be improved, as he did with many things which had not been patented. But he was quite prepared to let others make use of what he had done if he believed they had already been doing something of a similar kind.

Many attempts had been made by engineers to make an internal combustion engine work. Sir George Cayley in 1807 had clearly foreseen that an engine with a low weight per horse power ratio would bring the possibility of flight much nearer. In his notebook he described a small experimental engine in which gunpowder was used to move a piston. But he did not follow up the idea, having become obsessed by the hot air engine he had invented. Later Wenham was to be bitten by the same bug, as many other engineers were.

But before that, Wenham tackled the problem of the gas engine in his own way and perhaps his greatest disappointment during his engine work came following

his patent in 1864 entitled "Improvements in motive-power engines worked by explosive mixtures of gas or vapour."

He had done some experiments a few years before, experiments which he briefly described during a lecture meeting of the Aeronautical Society held on 15th July 1867.

"Some years ago," he said, "I constructed an engine worked by the explosive force of gas, and found that one-tenth of an inch of gas mixed with nine-tenths of air was sufficient to propel a free piston 12 in. and the mean vacuum obtained by the returned stroke amounted to 12½ lb. per sq. in. The propulsion of the piston was instantaneous, and the whole force of the explosion was afterwards utilised; the heat given out was so small, compared with the work done, that after running for ten hours the temperature of the cylinder did not amount to 300°. From this I infer that the cylinder chambers could be made to stand the heat of the explosion without being rapidly destroyed, and would make the lightest form of motive engine with which I am acquainted; the chief drawback would be the size of the reservoir containing the gaseous fuel, and bulk is one of the greatest obstacles to the success of Aerial Navigation."

In his patent he proposed to use two pistons, between which the explosive mixture was fired. One piston worked a crank by means of a connecting rod, while the other piston was free. When the explosion took place this second cylinder was held firmly, but in between explosions was free to move closely to the crank piston. The explosive mixture entered the narrowing space between the two pistons and was fired at the correct moment by means of a flame through a touch hole in the side of the cylinder. The lighted flame jet was enclosed in a coil of platinum wire which retained its heat sufficiently after each explosion to re-light the gas jet, if it had been blown out. An outlet valve in the cylinder allowed the products of combustion to escape.

"A further improvement," declared Wenham, "consists in passing the gas or air, either separately or together before entering the working cylinder, through a receptacle containing naphtha, petroleum oil, or other volatile liquids capable of affording inflammable vapour, which receptacle may be heated to assist the vaporisation."

Although Wenham and his partner had begun the manufacture of the engine, he was again faced with the worry that his claim to priority would be disputed. Lenoir in France had constructed a similar engine but it was very extravagant in fuel, and though a number were in use for some years they were soon superseded when the Otto cycle became a practical proposition.

In 1867 Wenham turned his attention to the heated air engine, which already had sixty years of history and endeavour behind it, and took out two patents. He made considerable progress with his ideas and on 1st May 1873 C. W. Cooke read a paper to the Institution of Mechanical Engineers under the title "On Wenham's Heated Air Engine."

Cooke gave a brief history of the development of the engine beginning with the words "The history of the Heated Air Engine dates as far back as the year 1807 when Sir George Cayley invented the engine . . . ."

In the report of the lecture, at which Wenham and a number of distinguished engineers were present, it was stated, "A model of Wenham's engine was shown, and one of the engines at work in the neighbourhood before and after the meeting, together with the Siemens electrical pyrometer by which the temperatures of the heated air were ascertained."

In reply to the many questions which were asked him, Wenham explained that there were three sizes of engines, 12, 15 and 24 in. cylinders. The smallest was abandoned after a while as it cost as much to make as the 15 in. Originally the 12 in. cylinder engine gave one half h.p., the 15 in. two h.p. and the 24 in. four h.p. "Several of the largest engines were now at work," explained Wenham, "and some of them have been running for five years."

In April 1882 Dugald Clerk lectured to the Institution of Civil Engineers on "The Theory of the Gas Engine" and Wenham took part in the discussion in the very same room in the Institution of Civil Engineers where Cayley himself had spoken in a discussion on the same subject in 1845.

"I have tried many experiments with heated air or gas engines, and have come to the conclusion," he said "that for economy and efficiency they surpassed steam engines for small power. In engines I made myself some years ago the air was pumped into a closed chamber which contained a coal fire, or in which petroleum was ignited by being injected on to an incandescent surface, and the expanded air was then conveyed through a passage and valves into the working cylinder, which was single-acting.

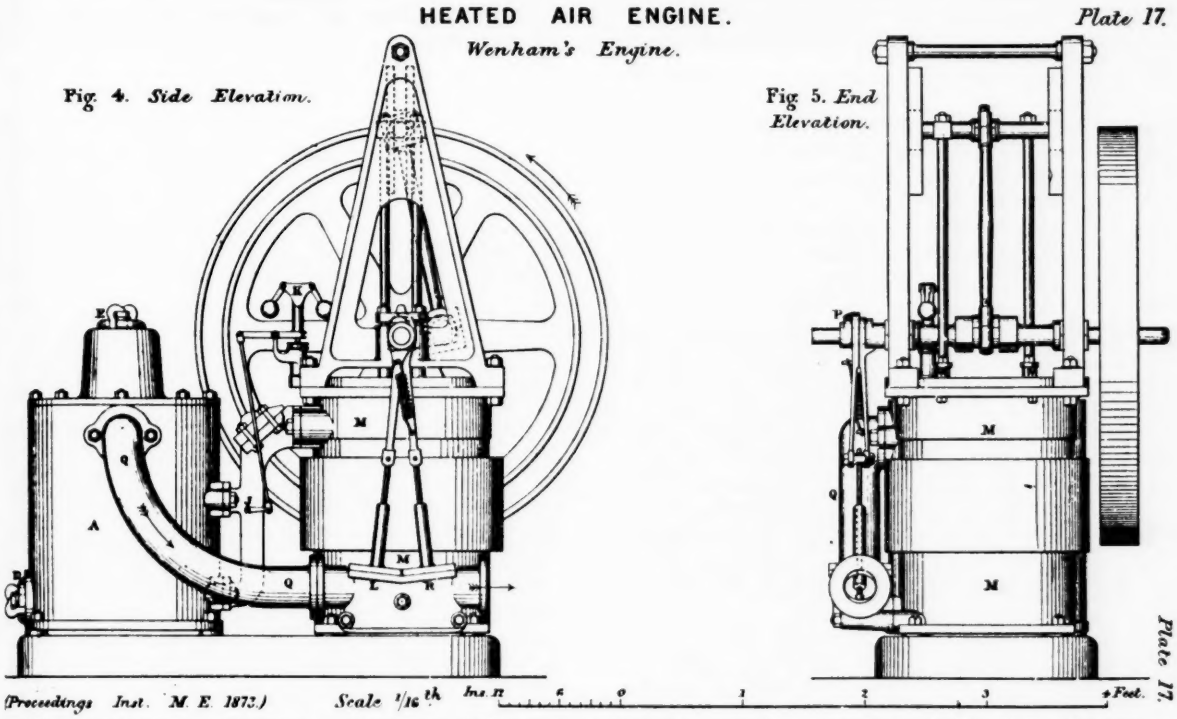
"A great many of these engines were made at the time, but there were practical difficulties in the way of construction.

"Under the conditions the air entered the cylinder at 1,100°F, equivalent to iron at a dull red heat and left at the exhaust at 466°, or a few degrees above the melting point of tin . . . . At its first introduction there was a considerable demand for the engine; but it has since been superseded by the modern gas engine . . . ."

Wenham drew attention to the great waste of heat at the exhaust, and said "In the future improvements of the gas engine would be such that such waste heat would be utilised as a motive power," a significant prophecy.

In June 1875, following a lecture by Thomas Moy on Aeronautical Progress to the Society, Wenham showed how anxious he was to find ways of lightening the great weight of engines at that time.

"I can give Mr. Moy a little information on a light engine," he declared. "I have prepared a condenser. The exhausted steam is passed into a chamber where 15 cubic feet of air are sufficient to condense one foot of steam. The condenser is very light, the walls being made of tin plate. Condensation is obtained simply by mixing steam with air, in a way that seems never to have been practically done before. Mr. Moy, as a practical man, will see that such a condenser can be made of the thinnest metal, tin plate in fact."



WENHAM'S HEATED AIR ENGINE:—*A*, furnace or air heater; *J*, air-flow valve control; *K*, governor controlling *J* ("so the combustion of the coal is exactly proportional to the amount of work performed"); *M*, cylinder. Heated air passes along *Q*, and is admitted to bottom of cylinder through valve *L*, opened by cam *P* on the main shaft. *R*, exhaust valve. The chief peculiarity of Wenham's engine was the arrangement by which the top of the cylinder serves as the air pump and is made to deliver into the furnace, for expansion, the reduced bulk of compressed air required for performing the work.

The possibility of the coming of the motor car again roused Wenham's interest in engines, after a long interval during which he had been busy in other spheres of engineering and science, which are noted in other pages.

In 1896 he filed two patents, the second of which was entitled "Improvements in mechanism for driving auto motors or self moving carriages." I quote from the Patent, No. 6177.

"If oil, vapour or gas engines are used for propelling carriages, the speed cannot be regulated by primarily altering the speed of such engines nor can the carriage be brought to a standstill by stopping the engine, and as engines of this class cannot be immediately restarted or reversed, they must be kept running and backing, stopping, and variations of road speed must be effected by gear.

"According to my invention I use two conical rollers preferably of the same size and angle, rotating on axes with the large and small ends together. The first or driving cone is rotated by the engine either directly or by a belt or belts from the main shaft. Either the first or second cone is capable of being moved to or fro in its bearings by a lever or screw or other suitable means. Preferably surrounding the second cone is an endless ring or belt of leather, rubber or other elastic material.

"When one of the cones is forced forward by an endlong thrust it wedges the belt at any place between the two, with any required grip, causing the two cones to rotate in opposite directions. If the belt be gripped between the large end of the first or driving cone and the small end of the second cone the speed of the second cone is high, but if the belt be shifted towards the small end while the cones are rotating the speed is gradually reduced."

Wenham added that a spur wheel fixed at the end of each cone allowed for reversing the car!

He was then 72 years of age, an age when even the most energetic of inventors normally would be thinking of retiring.

Microscopic Adventure

Wenham's place in aeronautical history is a high and world-wide one, but his place in the history of the microscope is at least as high.

Wenham was a controversial figure in the sense that he was not prepared to accept anything just because it had always been so and still was.

He had from early years shown a deep interest in optics, and especially telescopes and microscopes. He was only 26 when he was elected a Fellow of the Microscopical Society. "We first met him in 1850," wrote his biographer in the Journal of that Society in 1908, "when he brought before the Society a metal parabolic reflector." The Microscopical Society still has one of

"these very rare pieces of microscopical apparatus, the first made, now in the cabinet of ancient instruments."

The same year the young Wenham read a paper on the application of binocular vision to the microscope. This was a new idea then, enabling both eyes to be used. Each eyepiece allowed one half of the object to be seen.

Lectures, papers, ideas and correspondence poured from him. He was not in the slightest degree afraid of the old stagers and lashed out when they decried his ideas. In 1854 he published his paper on the theory of illumination under the microscope. It was strongly criticised. Only a month old member of the Microscopical Society, and but a comparative youngster himself, he retorted in May of that year, comparatively gently, with a paper which began, "As I have no predilection for a theory that does not bear upon a practical result, in that which I bring forward I shall endeavour as much as possible to support it by experimental facts . . . . As I do not agree with Mr. Bailey in the inference that he has drawn from observed facts, and the hypothesis that he has assumed is contradictory to the theory that experiment has led me to adopt, I find it necessary to make some comment on the contents of this paper."

It was the cooing of a controversial dove for 13½ pages and illustrations, to drive home his argument. Some comment! By an angry young man, in modern terms.

In the following year he published a paper on the same subject in the Quarterly Journal of the Microscopical Society.

"A perusal of this in the light of modern knowledge," wrote Edward Nelson in the Transactions of the Society in 1908, "is of interest . . . .

"'On page 152,' he says 'There is one peculiar phenomenon attendant upon oblique illumination at certain angles in one direction and may be described as a double image . . . . I sought for an explanation in the known laws of the diffraction of light, but these did not account for it . . . . I have since traced the cause entirely to the mutual dependence of the angles of illumination and aperture detailed in this paper. One image is caused by the radiations from the object entering one portion of the object glass and a different one by the object being directly seen by the intercepted light with the other extreme of the aperture, thus giving the appearance of a double image. . . .'"

"If Wenham had only assigned the origin of the phenomenon to diffraction," comments Nelson, "and the overlying image to spherical aberration, he would have anticipated much of what was to follow twenty years later."

The next twenty years had plenty of fight in them as far as Wenham was concerned!

Quite casually, in a lecture in 1855, he began, "As I have admitted that I take up the microscope only occasionally as a means of recreation rather than a special study, it may probably appear great assumption in one, comparatively unpractised, to make a statement of investigations that stand in opposition to the opinion of distinguished observers who have written upon the self-same subject after years of observation."

And then came the attack on some of the distinguished observers.

"I cannot refrain from protesting against the excessive predilection that most professional writers exhibit, in applying technical phraseology to a new branch of science even in the earliest stages of discovery. From this mistaken display of scientific pedantry, the uninitiated are probably at first quite unable to comprehend the matter; and worse than this, an inappropriate name is frequently applied to a circumstance or imaginary substance, having a doubtful or at least not a very general existence, and this name is continued throughout the whole series of subjects to which science relates, often giving rise to a maze of hypothesis. I make this remark because the physiology of the cell is an example of this."

Wenham was then lecturing on "The formation and development of the vegetable cell."

Wenham was a first-class mechanic who was not only capable of designing what he wanted but of making it. He made, ground, and polished his own lenses, down to the smallest, and published in 1869 five papers upon the practical construction of microscope object glasses for amateurs. He was not a very good mathematician, and never claimed to be, but in his papers he explains how he designed his object glasses by graphical methods and trial and error results, methods taught to him by J. J. Lister. The paths of the various rays were plotted on an enlarged diagram of the object glass. But this method is only accurate up to a point and for axial rays a considerable knowledge of mathematics was required.

"This lack of familiarity with elementary mathematics was a cloud which obscured his vision on many important points," wrote Nelson. "If only it could have been lifted, what an inventor he would have been!"

It is all the more remarkable, handicapped as he was mathematically, how much Wenham contributed towards the improvement of the microscope. The binocular microscope became for a time very popular both in America and Great Britain, until the improvements made by Professor Abbé. It was due to the work of Wenham, who invented the oil immersion system which proved such a great step forward in microscopy and led to the heated discussions over the years and ultimately to his resignation from the Microscopical Society. But he failed to appreciate the great importance of the system, which Abbé's mathematical ability enabled him to develop to its full extent.

Wenham was the first man to measure the aperture of an object glass with a glass apertometer. His statements about aperture started one of the longest arguments and most fiery discussions in the history of the Microscopical Society, with an amount of personal feeling which was afterwards regretted by every one, for it led to his final rupture with that Society in 1879.

Whatever those who were against him may have thought of his ideas and ability, the Council elected him as Vice-President in 1870 and again in 1873. The acid test of his ability came when Ross and Company appointed him their optical adviser in 1870, a post he

held until his retirement due to ill-health ten years later. The post was not one of his seeking, but was pressed upon him by a close friend who understood his natural flair and ability in a branch of science in which he was entirely self-taught.

Wenham's theory of aperture measurements could not be upheld, but the fact that it was wrong does not detract from the work he did and the ideas which he so freely gave to his audiences. He was against trade secrecy, and in 1871, although the adviser to Ross and Co., makers of optical instruments, he did not hesitate to publish in the papers of the Microscopical Society a severe attack on the way makers of object glasses kept information secret, something he always refused to do. A correspondent, who had asked him about how to grind diamond points, received a full reply, which ended, "I hope this contains a hint which may be of service to you. You can make any use of it that you think proper. Publish it if you like, for these little dodges are too often kept as trade secrets, to the detriment of their utility."

All his life, however, Wenham felt very strongly on the problem of the proper way to measure the aperture of an object glass, and in 1894 he wrote a long editorial in the *English Mechanic*. Over forty years had not softened his feelings or his opinions.

"I hold that the list of numerical apertures since published is entirely falacious," he wrote, "and not representative of the actual angle of aperture really appertaining to the object glass."

Much of the editorial was technical, and once more an assertion of his earlier arguments.

"The above facts," he added, "resulting from practical experiment I embodied in a paper which I laid before the Royal Microscopical Society. This was not written in the first person singular and was in no sense controversial; but as different views were advocated by some present in Council, my paper was peremptorily rejected and returned to me, without even a request for a demonstration. In consequence of this I tendered my resignation . . . ."

The breadth of interest which he had in the actual use of the microscope was shown by the many papers he gave and discussions he had on the actual structure and development of various objects he studied under the microscope, as the diatom, that curious single-celled plant of the sea-weed (and fresh water weed) family, the cells of which are joined together in a kind of zig-zag chain; insects, as those odd ones which jump by means of their tails, and so are named spring-tails, and so on. Wenham had the fundamental curiosity which was roused particularly, and at its best, by curious things. To him the microscope was not only an optical apparatus which he thought could and, indeed, did improve in many ways, but one which gave him great personal pleasure to use. From using it, as a brilliant amateur, he not only learnt the drawbacks, but he learnt how to give pleasure to others by describing what he had seen.

An Inventor's Sidelines

Wenham, like Cayley, was always ready to help anyone interested in engineering or science. It was in 1839 that a Royal Charter was granted in the name of Sir George Cayley for the Polytechnic Institution in Regent Street in London. He had founded the Institution for research work and the teaching of engineering and science, and especially for drawing the attention of the public to new discoveries in both subjects.

On his return from Egypt, Wenham was asked to take an interest in a similar Institution called The Panopticon of Science. It was built in the Moorish or Arabesque style and opened about the same time as Wenham was preparing to go up the Nile. It was founded by a number of philanthropists and was one of the most "elegant places of entertainment of the kind in London." There Wenham constructed a special apparatus for the enlargement of photographs, a process which was little known at the time. But the Panopticon did not prove successful. It afterwards became one of London's most famous Music Halls, the Alhambra.

Photography in the 1850's was a formidable process. The Daguerre method enabled photographs to be taken until it was succeeded by the collodion wet plate process which was used when Wenham took Frith up the Nile. As its name implies, a plate had to be exposed while the emulsion was still wet. In hot climates a photographer's difficulties, delays and troubles were increased by the speed at which a bottle of collodion could evaporate!

In 1853 Wenham described his method of enlarging photographs in the *Journal of the Royal Photographic Society*, which he joined five years later. His paper was a long one with the fantastic title "On a method of obtaining large positive impressions from transparent or albumen negatives by means of an ordinary photograph camera"—nowadays "Enlargements."

Wenham disarmingly began by disclaiming any originality for his method of enlargement, "as a nearly similar plan for enlarging photographs had been proposed at an early stage of the art."

The enlargements depended upon using the camera lens itself to provide the enlarging apparatus. Through a hole in the shutter of a dark room, sunlight was reflected by a mirror through the plate and camera, the reflecting mirror being adjustable to allow for the movement of the sun's light.

"Hitherto photography has been almost confined to miniatures," wrote Wenham, "but if this new branch of the art be cultivated by experienced manipulators, I see no reason why, in their hands, landscapes may not be enlarged in the same way . . . . There are many useful applications of this process that will suggest themselves, for example, obtaining photographs of machinery, enlarged to a given scale for working drawings, some specimens of which I hope to have the pleasure of exhibiting to the Society at an early opportunity . . . ."

Wenham retired from Ross and Co. at the age of 56—but not from work. He turned his attention to gas lamps, and particularly to the Argand gas burner.

In 1882 he filed his first patent which was followed by a number of others in the next few years. His first object was to improve the intensity of the lighting by heating the air or gas. He followed this by the inverted burner, or "shadowless gas lamp," which became widely used in the gas-lighting field for many years, both for street lighting purposes, and for indoor illumination.

His health at this time not being too good, he retired from business activities, but continued to interest himself in anything mechanical, whenever he felt like doing so, which was more often than not. But I confess I was astonished when I read the title of Patent No. 5024 of 1884, "Improved mechanism for mechanically playing pianofortes and other musical instruments where notes are sounded by percussion."

This patent showed an improvement on the mechanism to lessen the strain, and hence the wear and tear, on the perforated music rolls for piano players, which were then in an embryo stage of development.

He was interested in acoustics, and particularly musical instruments. In 1892 there was a series of letters in the *English Mechanic* on "How to make a violin." Wenham not only joined in the correspondence but added, as a makeweight, an account of how to tune it when made, and how to obtain a good tone. As usual he looked at the problem from a new angle.

In a further letter he reminded some of his critics that twenty years previously he had written to the *English Mechanic* to point out how the tone of a violin could be improved and added that he had carried out tests, by varying the thickness of the wood used, to enable the belly and back of the violin to vibrate in unison.

Aeronautical Anticipations

Although to F. W. Brearey, the first Honorary Secretary of the Aeronautical Society, great credit is due and has been given for the actual first steps which led to the foundation of the Society, greater credit during the early years must go to F. H. Wenham than to any other man. Wenham was an engineer with strong leanings towards applied science. Brearey had no technical qualifications whatever, but possessed an enthusiasm beyond the ordinary. In most references to the early beginning of the Society, Wenham's dominating position has been overlooked simply because his own career, *outside aeronautics* had not been considered interesting enough to be studied by aeronautical people.

Yet there are some interesting facts about the composition of the Council during the first three or four years which indicate the powerful influence of Wenham and his friend James Glaisher in the founding of the Society. In 1864-5, just before the Society was formed, Wenham was a well-known marine engineer, a member of the Microscopical Society and of the Photographic Society, and one who had for years been interested in the possibilities of heavier-than-air flight.

James Glaisher had long been a pioneer of meteorology, and had made many scientific balloon ascents on behalf of the British Association. He was President of the Royal Microscopical Society from 1865 to 1868, and President of the Royal Photographic Society from 1869 to 1892. So the two men were not only friends but shared much the same general outlooks and ideas.

Another member of the Microscopical Society who became a member of Council was Charles Brooke, well known for his work on self-recording instruments. He also became a President of the Microscopical Society, as Wenham became a Vice-President. John Spencer Browning, who helped with the first wind tunnel experiments, was a Fellow of the same Society when he became a member of the Council of the Aeronautical Society. One may well say, indeed, that the Society began from microscopic beginnings and has now become macroscopic. A sixth member was the famous marine engineer William Fairbairn, President of the Institution of Mechanical Engineers in 1854, and of the British Association in 1854, and a seventh Sir Charles Bright, who did so much to make the laying of the Atlantic submarine telegraph so successful.

At the first meeting of the Council Wenham was appointed, with James Glaisher and the Honorary Secretary, to draft out the Rules of the Society, rules which were not altered until 30 years later. He was quickly to play an active part and to serve on a number of Committees. Before the year was out he found himself, with William Fairbairn and others, on a Committee which was appointed "to discuss on the best means for causing a balloon to ascend and descend, either by mechanical or chemical aids, so as to economise the use of gas and ballast." For whatever may be said, that first Council recognised that if a balloon could be controlled in the air, and not be entirely dependent upon the winds which blew, a lighter-than-air vessel must then be classed as one type of flying machine and the heavier-than-air as another.

At the same meeting of Council at which it was agreed to consider controlling the balloon, Wenham also offered to serve, with Glaisher and James Nasmyth and others on a Committee, "To consider the law of resistance of inclined surfaces in elastic and non-elastic fluids as air and water, and the resultant force obtained at right angles to the direction of motion. Experiment has now proved that as the velocity is increased, and the angle of inclination diminished, the centre of effort of the plane approaches forward, till nearly the whole effect of reaction or support is transferred to the front edge. It therefore follows that in very acute angles with the line of motion, a given extent of reacting surface at high velocities will be most effective if disposed in a narrow plane extended transversely to its direction.

"An investigation of this principle will determine the best ratio of surface to speed in the long and narrow wings of birds of extended flight; in the blades of reciprocating and screw propellers, and in the sails of vessels for sailing near the wind."

This long extract from the Council Minutes of 24th August 1866, reflects the outlook of Wenham.

Less than two months before that meeting, the first members of the newly formed Society had heard him give his astonishing lecture, in which he had drawn attention to the importance of aspect ratio; the effect of the resistance of the air to bodies passing rapidly through it; of wing loading and of weight per horse power; and of the value of a light structure. He had also outlined experiments he had made both to ascertain the lift and drag of surfaces and the efficiency of airscrews.

He had written a long letter to the *Mechanics Magazine* on the 1st June 1866, drawing attention to the formation of the Society and summarising some of the things he was going to say and deal with in his inaugural lecture at the end of the month. From January to June, before that first meeting, reports of the meetings of Council were published, and the names of leading engineers, scientists and others who were joining were given to show that whatever might happen in the future, a number of informed and trained individuals were of the opinion that the possibility of flying in a heavier-than-air machine was worth investigating.

The *Mechanics Magazine* reported on 20th April 1866 that Mr. Butler, a member of Council, had visited Paris and had discussed the possibility of a joint Aeronautical Exhibition in the near future.

The *Mechanics Magazine* of the same date reported that the first experiments under the auspices of the new Society would take place in May, "conducted by Mr. Glaisher and Mr. Webster, when in addition to observations of a meteorological nature, some experiments specially adapted to the confirmation of a theory as to the flight of birds by Mr. Wenham, C.E. should be attempted from the car of the machine . . . ."

The balloon ascent was made at the end of May from Windsor and the *Mechanics Magazine* reported, "Mr. Fred Brearey, Hon. Secretary of the Aeronautical Society of Great Britain, attended with some models presented by Mr. Wenham, to detach from the car of the balloon at the height of about half a mile, in illustration of the flight of birds . . ."

Alas, I have found no further details of these early experiments. Wenham did not mention them in his lecture or in the discussion which followed.

It is known that he made a number of isolated experiments in the years before the Society was founded, some of which he described in his lecture, "which was written seven years ago," as he told his audience. These experiments varied from investigations into the power which could be transmitted through an airscrew, to full-scale gliding experiments.

Undoubtedly Wenham's discussion on the effect of the long narrow wing as against that of the short broad one was felt at the time of the lecture to be an important step forward towards the solution of the problem of flight.

The Duke of Argyll, President of the Society, who was in the Chair, emphasised the point and Wenham, drawing attention to Cayley's statement in his paper of 1809, "In very acute angles with the current, it appears that the centre of resistance . . . . does not coincide with the centre of surface, but is considerably in front of it," made the comment, "From this discovery, it seems remarkabe that Sir George Cayley, finding that at high speeds with very oblique incidences the supporting effect becomes transferred to the front edge, the idea should not have occurred to him that a narrow plane, with its long edge in the direction of motion, would have been equally effective."

Wenham, however, like many experimenters after him, underestimated the difficulties of accurate measurements in the field of aerodynamics.

"To demonstrate the supporting power," he wrote to the *Mechanics Magazine*, "of inclined planes passing through the atmosphere (or met by a current of air) and to reckon the force applied to relative weight, is a very easy matter; yet this has attracted so little notice from scientific men that the experiments appear never to have been made, as there are no published results . . . ."

During all the years he was interested in flying, Wenham had realised the importance of the power required to fly and the transmission of that power through an airscrew. In his first lecture, in many papers and in discussions, he emphasised always the importance of the screw. His early training in marine engineering, when the screw was being first tried and was seen to be far more efficient than the paddlewheel, was to stand him in good stead in his arguments for its use in aeronautics.

At the meeting of the Society held on 17th April 1867, in a discussion on a paper by Henri St. Martin, Wenham began by raising his favourite topic, the screw. "This is, perhaps, in all respects perfect as a propeller for aerial machines . . . for the mere purpose of raising a weight, as in the model before us, I consider that it is utterly valueless, and that the misdirected attempts that have been made to apply it in this way have been one of the greatest drawbacks to Aerial Navigation. Of all things this is the most easily tried, and the least satisfactory in its result."

Wenham was adamant against the use of the ordinary airscrew to provide the necessary direct lift for a reasonable power/weight ratio.

"My own experiments, made with a vertical screw of fans of various forms, 6 ft. in diameter, driven by gear and accurately made, . . . could never raise or sustain a weight exceeding 10 lb."

He discussed contra-rotating screws on the same shaft and declared they were not satisfactory, as the lower screw was working in disturbed air from the upper screw.

Wenham worked hard, apart from speaking in lectures and discussions, to help the Society. Brearey had no technical knowledge so that Wenham and Glaisher had to provide the answers as to what papers should be considered at lecture meetings. Most of the actual work fell upon Wenham. There were eleven Council meetings that first year, all of which he attended, taking the Chair on a number of occasions. He acted as the Honorary Auditor, and Glaisher, who attended seven of the meetings, as the Honorary Treasurer. These two were indeed pillars of strength in those early years.

Brearey's expenses book reveals much of the time Wenham gave to the Society's work. The following are a few typical entries of expenses before the First World War, long before.

"Visiting Mr. Wenham at Clapham to consult him about the Report, 2/-; meeting Mr. Wenham in town to read over the papers for the General Meeting, and expenses, 2/6; to town to meet Mr. Wenham at request of Mr. Browning to see instrument, 1/-." (The "instrument" was the first wind tunnel ever made.)

Although Wenham often disagreed with papers submitted to the Society, and disagreed violently on occasion, he was convinced that as many ideas as possible should be ventilated, on the grounds that only the sound ones would survive. It was laid down that the Society, as a body, would express no opinion about the ideas of individual members, however mistaken those ideas might appear to some.

At the lecture meeting of 17th April, 1867, Wenham played a major part by summarising the state of Aeronautical Science as then known, and in taking part in the discussions on the papers read.

"Our knowledge of Aeronautics, as far as regards the navigation of the air by mechanical means, amounts to but very little," he said, "and the information recorded is of a contradictory character . . . . without a definite law of the acting and counteracting forces of the elastic air, we have not even entered the threshold of aeronautical discovery, and attempts at obtaining mechanical flight cannot be foreseen in their results . . .

"In the case of a bird we have the singular condition—not readily comprehended—of a body immensely heavier than an equal bulk of air, being easily supported, moving freely in all directions, and even in some instances almost living therein. The fish, the ship, or a balloon, may be nearly all the same weight, or lighter than an equal bulk of the sustaining fluid, and on the laws of displacement it is quite understood why they float; but here our ideas appear to have ended."

Wenham analysed, from a purely engineering point of view, the reasons why a bird was supported when in flight.

"The principle may be reduced to a differential action between two weights opposing each other. The bird when stationary or moving very slowly, must speedily fall upon the small weight of air acted upon, and yielding beneath it during a brief time, unless great exertion be employed in counteracting the descent. In the other extreme, during rapid forward speed, though the weight of the bird is in no way diminished, yet the relative weight of the air by which the body is upheld by the outstretched wings is enormously increased at the same time . . . ."

Wenham developed the argument, drawing attention to known laws of action and reaction between solid bodies under forces, and pointing out that similar laws must apply.

"In the present state of inquiry," he ended, "a series of experiments is much needed, in order to furnish the data for construction. Should these establish the law of the capability of an atmospheric stratum for supporting heavy weights by means of very slightly inclined surfaces travelling at high speeds, we shall then have a certain fact to start from; and let it be borne in mind that the air as a means of transit has in one respect an unequalled advantage, that of a ready made roadway, without hills, turnings, or irregularities to damage or break machinery; consequently, the only limit to speed is not in safety, but the amount of propelling force that can be applied."

It was a good thing, perhaps, that the aerodynamics of flight was still a mystery! Nevertheless, Wenham's insight into the fundamental dynamic problem to be solved was sound.

He added "I propose shortly to try a series of experiments by the aid of an artificial current of air of known strength, and to place the Society in possession of the results."

The year 1868 was that of the first Aeronautical Exhibition, held by the Society. It was a very active year for Wenham, for he not only spoke vigorously in most of the discussions at the meetings held during the year, but was one of the members of the Exhibition Committee, and drew up the report of the Exhibition that was published with the Annual Report.

To his report of the Exhibition Wenham added an assessment of to what extent it had forwarded the science of aeronautics.

"With respect to the question of mechanised flight," he declared, "it may be stated that we are still ignorant of the rudimentary principles which should form the basis and rules for construction."

He pointed out the absence of urgently needed experiment, and the necessity for a low weight per horse power ratio for successful flight.

The first lecture meeting of the year was held on 25th March and a Mr. Joseph Heath proposed to fit a balloon with an adjustable belt, rather like the belt round Saturn, adjustable in the sense that it could be inclined to the horizontal. He suggested that the flow of air over the surface of the belt, as the balloon ascended or descended, would enable the balloon to be steered to some extent.

Wenham was short and to the point. "I cannot see how any effect is to be obtained in this way from an abutment contained within the machine itself," he commented, and no more was heard of the scheme.

A paper which followed was by A. Alexander, a member of the Council and an engineer of some standing. He was the father of the Patrick Alexander who was to be such a great benefactor of the Society and of British aviation generally in later years.

Alexander spoke on "Power in relation to weight in Aerial Navigation." At the time he lectured, the lightest engine with its accessories weighed more than a hundredweight per h.p. Nevertheless Alexander came to the conclusion that flight was possible with such a weight. Wenham, during the discussion, stated that he had calculated the horsepower of a pelican as about 1/11th h.p., about half that of a man, for normal working conditions.

Wenham did not always appear to distinguish between power for flight and gliding. He believed that a man would be able to fly under his own power, once

he had learnt the art of making use of the pressure of the air. In the course of the discussions that year, he said, "I can see no reason why men should not fly a certain distance, and I have no hesitation in saying that if a man can raise himself in the air he could lower himself by the same means as gently as possible. In the experiments I have tried, I had always up to the present time failed to raise myself in a calm, but in a twenty mile wind I could raise myself without difficulty. In fact the last experiment I made was dangerous."

The year 1870 was a notable one in the Society's history and one in which Wenham played a leading part. At the May meeting of Council Sir William Fairbairn drew attention to the absence of data giving the "reactions and lifting forces" on airscrews, and expressed the opinion that experiments to obtain the necessary data would not be difficult.

James Glaisher told the Council that he also had been trying to obtain data concerning the forces on plane surfaces by using two anemometers at the same time. Out of the general discussion it was agreed that an experimental fund should be raised, and at the June meeting an experimental committee was formed. It included Charles Brooke and James Glaisher, both Fellows of the Royal Society, and four engineers of standing, E. W. Young, D. S. Brown, W. H. le Feuvre and F. H. Wenham.

The terms of reference were "To ascertain the fundamental relation between velocity and pressure on surfaces of different areas and inclinations."

At the end of the Annual Report of the Society for 1870 it was reported,

"For the purpose, an instrument has been designed by Mr. F. H. Wenham, and approved by the Experimental Committee, which it is intended to submit to the action of a fan not less than 30 in. in diameter, capable of delivering about 3,000 cubic feet of air per minute. A clear space of 15 ft. or more in front of the fan will allow room for a square wooden trunk to guide the blast, ascertain its velocity, and insert the anemometer."

Francis Herbert Wenham can justly claim to have designed the first wind tunnel, primitive though it was, expressly for aeronautical purposes. It was the very beginning of the development of the instrument which is fundamental in the research for an understanding of aerodynamic progress.

In the 1871 Annual Report of the Society some particulars of Wenham's tunnel and experiments were given and the recorded measurements of the forces on planes at various angles of incidence to the wind were published.

"By means of a fan blower, a current of considerable force was directed through a trunk ten feet long by eighteen inches square."

It was stated that as a result of the experiments, although they were "somewhat crude and incomplete," that—

"We have no longer the extravagant and impossible theories of gravity and the laws of motion to account for this difficult phenomenon, so many of which were sent to the Society in the early days of its existence, the publication of which would in no way have contributed to the end in view, but, on the contrary, being wholly unsupported by any previous facts or experiments, would have brought discredit and ridicule upon Aeronautics, as the science which it may fairly be considered.

"Should the problem be solved, the Aeronautical Society will take a high rank among its compeers . . . . The Society may congratulate itself for having been the means of bringing together and recording a number of facts not lost upon its members, which have produced greater unanimity of purpose, and have, it is presumed, directed the efforts of experimentalists in accordance with a more general and practicable theory."

Self-praise may be no recommendation, but one must feel very glad to read what Wenham placed on record eighty-six years ago about the Society and its first attempts at experimental research.

At the Lecture meeting held on 18th April 1872, in the Society of Arts, where most of the lectures were held for the first seventy years of its existence, the wind tunnel was shown to the members and its working explained to them by Wenham. James Glaisher, introducing the lecturer, was clearly feeling this was a great occasion.

"The subject which will naturally attract our attention this evening," he began, "is that of the experiments which have been made by the apparatus on the table before us . . . . It is an instrument of a kind which I have long desired, and it seems calculated to achieve what we require in this direction with greater accuracy than any other instrument I know. I call upon Mr. Wenham to explain the apparatus."

Here is Wenham's account.

"To make the instrument understood, I must explain how it acted as an ordinary anemometer, for ascertaining the direct force of the wind on a plane, when in a vertical direction to its surface.

"This consists mainly of a vertical steel spindle, supported on a hardened steel centre. Through an eye at the upper end of the spindle, a horizontal arm passes, and is secured by a small cross-pin, which allows the arm to vibrate like the beam of a balance. The long end of the arm carries the planes, and the opposite short one has a sliding counter-weight, which is adjusted so as to exactly balance planes of different sizes at the long end of the arm. Each plane is clamped at the end of a tail rod, which is pivoted through the forked end of the arm, by a vertical steel pin, as close to the plane as possible; the other end of the tail passes loosely through a vertical slot, slightly curved as a radius, from the balance centre of the arm.

"By this arrangement the surface of the plane is always kept at right angles to the current, through the extent of its horizontal motion. A wooden shield is fixed close before the front of the arm, to protect this and the balance weight from the wind, so that the planes only may be exposed to its force. The action of the instrument, as a single anemometer only, or when the planes are set at right angles to the current of air, is obvious. The direct pressure is read off by the spring steelyard, which is connected to the end of a lever from the vertical spindle, close to the base of the machine.

"In order to measure the vertical forces, the planes are set at the requisite angles from a divided sector, whose centre coincides with the clamping screw at the back. The raising force due from the various inclines, was read off by the upright steelyard. It was found almost impossible for one observer to read off the horizontal and vertical forces simultaneously during fluctuations, therefore the readings were noted by two persons at a given signal—even this was a matter of some difficulty.

"The arrangement would be far more useful and perfect as a scientific machine, if fitted with a piece of clockwork, moving a paper cylinder, on which the vertical and direct forces would be simultaneously registered by separate pencils, describing two undulating lines, showing at a glance the relative forces; the experimenter would then have nothing else to attend to, but to see that all other conditions were acting properly."

James Glaisher and Charles Brooke, who had both taken an active part in the experiments, pointed out the difficulties of obtaining close accurate measurements.

"Anyone who has not considered with care the nature of the pressure produced by the flow or rush of a fluid," said Glaisher, "elastic or incompressible, against a plane surface placed in its course, might imagine that the system of parallel forces was merely equivalent to a single resultant force acting at the centre of pressure, and capable of resolution according to the ordinary parallelogram law. But this of course is not the case, for the particles of the fluid which come in contact with the plane, have somehow or other to get out of the way, by gliding along the surface of the plane (as they cannot get through it) and this produces a complication in the neighbourhood of the surface of such a kind as cannot be theoretically predicted. One thing, however, is quite clear, and that is that the directions of all the small forces acting on the surface certainly are not parallel, and we must therefore have recourse to experiment."

Wenham was eagerly questioned. One member began by asking if, while the experiments were being made, any attempt had been made to find out if there was any downward pressure of the air current.

"No," replied Wenham. "I omitted to mention that. A spirit level was laid across, so as to level the instrument (*i.e.* the tunnel itself). We had a trunk twelve feet long and eighteen inches square to direct the current horizontally, and in a parallel course."

Asked if he had noticed the movement of the centre of pressure, he replied "We were not able to ascertain very accurately. In all cases there was a tendency to lift the front edge. We found as the angle became more acute, the centre of pressure came nearer the front edge."

These experiments did not attract the attention which they deserved but, ten years later, in March 1882, they received an unexpected and deserved tribute from Professor W. C. Unwin, when speaking in a discussion on a paper by C. B. Bender at the Institution of Civil Engineers. Bender had been speaking on the design of structures to resist wind pressure. Unwin himself had lectured at Chatham in 1868 on the effect of wind pressure on roofs. He related that he had worked out a rule for making a proper allowance for wind pressure on a very long roof for a riding school.

"The engineer most responsible for its construction thought I must be mistaken," said Unwin. "I mention the matter because I had found some experiments which had not been quoted in that connection, but which appeared to be most valuable in relation to the pressure of fluid on inclined surfaces.

"Just before the time to which I refer, Mr. Spencer Browning and Mr. Wenham made some experiments in Messrs. Penn's works at Greenwich for the Aeronautical Society. They took a plane 2 feet square and placed it opposite the mouth of a large blowing fan, and they arranged to measure the two rectangular components of the pressure of the plane, the pressure in the direction of the wind and at right angles to it.

"All the earlier experiments measured only one component, from which the normal pressure on the surface had to be inferred. Combining the components measured by Mr. Wenham and Mr. Spencer Browning, the resultant pressure on the surface was found to be, as it should be, almost exactly normal to the surface. The results on an inclined surface, down to an angle of 15° agreed almost exactly with Hutton's rule, or still better, with a rule I should now prefer, mainly on the ground that it was very simple, the rule of Colonel Duchemin."*

And so ended the first seven years of the Society's existence.

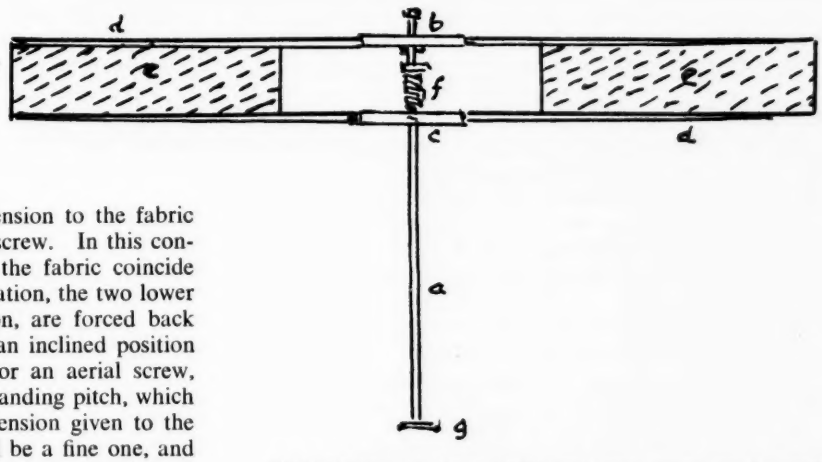
In the summing up of the year's work at the end of the 1872 *Annual Report*, there was put forward a method, due to Wenham, for varying the pitch of an airscrew.

"One difficulty has been a ready means of varying the angle of pitch of the screw, in order to suit the velocity of rotation and the force applied," said the *Annual Report*. "Mr. Wenham has proposed a simple kind of screw for this purpose, constructed in the following manner:—*a* is a hollow spindle or tube, at the end of which is fixed a cross-socket *b*, with two arms. Sliding on the spindle loosely is a similar socket, *c*. Into the ferrules of these two pairs of sockets, taper flexible wands, *d*, *d* are thrust; these are shaped like billiard cues, and made of light elastic wood. From the extremities of these, to near one-third the distance towards the centre piece of fabric, *e*, *e* is sewn between them.

"A light rod passes through the hollow spindle, having a short cross arm at the outer end. Two return rods from this afford the means of compression to a spiral spring, *f*, surrounding the spindle, and resting on the sliding socket, *c*. At the lower end of the spindle there is a cross handle, *g*, tapped to receive the screwed end of the inner rod. By turning this handle the spring is compressed, forces down the lower sliding socket.

*If P = pressure on inclined surface, P' pressure on surface at right angles to the wind, α the angle of inclination, then Duchemin's formula is

$$P = P' \frac{2 \sin \alpha}{1 + \sin^2 \alpha}$$



Wenham's suggested method for varying the pitch of an airscrew.

and of course gives any required tension to the fabric connecting the rods or arms of the screw. In this condition the four arms or planes of the fabric coincide with the axis, but if this is set in rotation, the two lower arms and socket, being free thereon, are forced back by the resistance of the air, giving an inclined position to the fabric of the proper form for an aerial screw, with a somewhat hollow face or expanding pitch, which can be exactly determined by the tension given to the spring; if this is slack the pitch will be a fine one, and when screwed hard up the lower socket will yield but little, and a coarse pitch be obtained.

"As the rods twist or deviate from each other, of course the connecting distances between them become greater at the extremities than near the centre. This is compensated for—1st by leaving the middle as an open space; 2nd by having the fabric loose at the extremity, so as to meet the coarsest pitch required, and 3rd the rods being properly elastic at the ends, yield so as to stretch the fabric uniformly in fine pitches, giving the blade of the screw a taper form, which is not an objectionable one, but the reverse."

In June 1880 R. A. Proctor, a friend of many members of the Council of the Society, wrote a short article in the *English Mechanic* on the flight of certain sea birds.

There followed a remarkable series of letters, spread over many months, from members of the Society and others, who welcomed a weekly platform on which to argue, as against the long intervals between the Annual Reports. Wenham's ideas were written about by some of the writers, but it was not for some weeks that he decided to join in. In his letter he pressed the importance of the speed of a wing through the air for support, so giving a smaller gliding angle if the wing is allowed to descend. One persistent writer wrote that he had small veneration for the Aeronautical Society, and "almost everything I read issued by members of the Society lessens my confidence." He added that "the theory of the descent of bodies has nothing whatever to do with aerial navigation, whose object is to keep a body from descending at all for a long time!"

Many correspondents disagreed with Wenham, who had stated that, in effect, the lift of a plane at a very small angle of attack, increased with speed. In his reply to his critics Wenham pointed out that no experiments to find out had been made, "beyond some crude experiments made by myself some years ago. I have done nothing further to solve the question."

He was politely telling them that he, at least, had done something, which was more than could be said of his critics.

There is not space here to quote the correspondence, which began in June 1880 and ran every week until December. Most of the writers were clearly puzzled by the problem, for they believed the weight of a large bird was too great to allow it to soar under the normal laws of mechanics.

A number of correspondents drew attention to the flight of the sycamore seed and Wenham, in a letter on on 27th August 1880, made the interesting comment that he had made some experiments with the seed. "A trial of samples will prove that, if dropped from an elevation, some take a long time to reach the ground, and these are the largest and heaviest from amongst them. If the seed be weighed and the time of descent from a housetop noted, it will be found that the retardation of its fall is duly proportional to the area covered by the rapid revolutions of the seed, and corresponds with the air resistance (*i.e.* lift) of such a surface. This I ascertained many years ago."

Wenham spoke with an experience which most of the writers could not emulate, and just to show that he had not forgotten his long connection with the Microscopical Society, he added, before the end of the year, two letters on the properties of glass and microscope lenses and a third, in the early part of 1881, on eye pieces for telescopes.

In 1882, at the July meeting of the Council, it was reported that Wenham had resigned. It was a severe blow to the Society. There appeared no more of those technical comments which had been made year after year by him in the Concluding Remarks of the Annual Reports. For long Wenham had been at loggerheads with the Hon. Secretary, as indeed had James Glaisher, chiefly on account of Brearey's lack of technical knowledge and his assumption he knew better than those who had. Only one more comment on the Society's work appeared in the Annual Reports and that was by Brearey, signed by him for the first and last time, for that year 1893, was the last time the *Annual Report* was published.

Brearey died on 31st January 1896. In 1896-7 Baden-Powell took over, and revived the drooping spirits of those who had remained faithful in the face of so much indifference. In September 1899 he wrote to Wenham, on behalf of the Council, and offered him

Honorary Membership. In his reply, accepting the honour, Wenham explained his reasons for leaving a body and its Council which he had served so well.

"I sent in my resignation principally for this reason," he wrote, "Your late Hon. Sec. (deceased) *patented* a fantastic idea of a flying machine consisting of a long waving sheet, and was offended at me for not praising it as being the right thing for ensuring ultimate success. I got letters from others blaming me for not *condemning* the scheme and so have saved them some hundreds of pounds which they had been induced to invest and so lost by the patent. This was very annoying to me especially as there were threats of a law suit in which I was required to give evidence.

"Fifteen years ago I retired from business and bought ten acres of land on the top of this hill . . . . The spot would serve for a gliding flight, but is so wooded with large trees and evergreens which cause eddying currents of winds that I find much difficulty in getting a steady breeze for outdoor experiments."

Wenham never lost his interest in Aerial Navigation. Indeed, his last official letter was written to the Aeronautical Society less than a month before his death and one of his last personal letters was to a member of the Society on aeronautics the week he died. His last article was published in the *Aeronautical Journal* which announced his death.

Busy as he was in other fields, after his resignation, he remained one of the best known aeronautical figures in those pioneering days, and was called upon from time to time for an authoritative opinion or statement upon some aspect of aeronautics. In November 1891, for example, the *English Mechanic* asked him to write the page leader, which he did under the title "Some remarks on aerodynamics."

It began, "I have received an elaborate article consisting of 115 pages by Professor Langley under the title 'Experiments in Aerodynamics' published by the Smithsonian, August 1891, No. 801 . . . . The results are beyond the reach of adverse criticism, as they speak for themselves . . . . the essay appears as the dawn of a new era, and anyone occupying himself with the problem of flight by man, can no longer be subject to ridicule of the 'impossible,' always the ready remarks from those who know nothing about the matter . . . .

"During the last twenty-five years considerable attention has been directed to the subject of flying machines and the flight of birds, the cause of which has been attributed to all manner of fantastic actions and wing movements such as the air passing through the interstices of the feathers—in the upstroke of the wing, which was supposed to close like a valve on the down stroke.

"It has been suggested that the wing describes a figure of eight as a necessary condition of flight, or that flying is to be accomplished by loose undulating surfaces. All such suggestions are baseless. To attempt to imitate the construction of a bird's wing and the mechanism that actuates it. is an irrational if not impossible task; yet curiously one of the first adaptations to a steam locomotive was an imitation of the reciprocating action of a horse's legs for propelling a machine."

Space is not available to say all that Wenham said so cogently in this editorial. The greater part of it discussed and described and praised Langley's experiments, for here, at long last, were those very experiments for which he had urged over the years, ever since the founding of the Aeronautical Society. He mentions briefly his own 1866 paper "and as I did not fly, my paper attracted but little attention, and the principles enunciated therein were ignored and not generally understood . . . ."

For many years Wenham had been a friend of Octave Chanute, the great American railway engineer and friend of the Wright Brothers. It was Wenham, indeed, who first interested Chanute in aviation when he was on a visit to England in 1875.

In 1893 Chanute wrote a long series of informative articles in the American *Railroad and Engineering Journal* on Progress in Flying Machines. In it are many quotations from Wenham. At the beginning of August of that year there was held in Chicago an International Conference on Aerial Navigation of which Chanute was the organising Chairman. Under his vigorous direction nearly forty papers on aeronautical subjects were presented. There was a feeling in the air that its conquest was not very far off, and many of the papers reflected the views of the time of engineers and scientists and research workers who were interested in the possibilities.

It was not unnatural that Chanute should remember his old friend Wenham, and ask him to contribute a paper for the Conference. Wenham did so on "Suggestions and experiments for the construction of aerial machines."

In this paper he again reiterated, at some length, as he had done on so many occasions, the importance of the leading section of an aeroplane wing and of the advantage of the long narrow wing in consequence, or its equivalent.

"On reviewing the very numerous suggestions and actual constructions of flying machines it is remarkable how these elementary conditions have been ignored or not even comprehended. In many of the subsequent wing arrangements for flying machines the very opposite form has been adapted, with the supporting surface extended back lengthways, having the narrow end in front—a very hopeless form for the purpose in view.

"Generally, for the sake of illustration, the supporting surfaces have been spoken of as 'planes,' but for a suitable application in an elastic yielding medium like air, the form expressed by this term is not strictly correct. The surfaces should have a form with a progressively increasing incline, known as the 'expanding pitch,' as adopted in the best form of screw propellers. The requisite increase of pitch for elastic air is far more rapid than for inelastic water.

"If we take a strip of fabric having some weight of material—preferably with a smooth surface—such as oilcloth, and trail it against a current of air, it will extend outward in nearly a parabolic form, thus: (*See* Figure.)

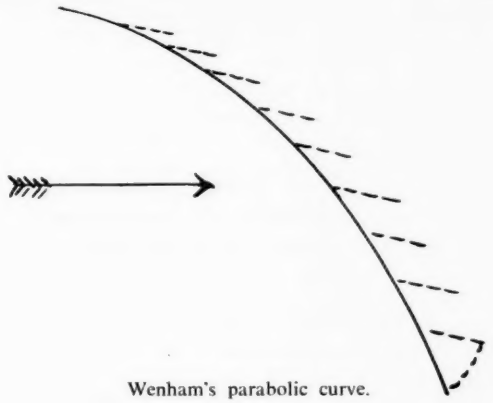
"The stratum of air caught by the top edge becomes immediately deflected downward, and deviates in a different direction, by which the underlying position is influenced and follows in like course, giving a special form of curve to the fabric. We have here but little supporting effect, this being principally confined to the top edge. The after part of even a curved surface of this description is quite ineffectual for sustaining weight. The pull or forward resistance caused by the extension is very great. The curve in its continuation to its lower end takes the direction of the deflected current of air, or nearly so. But suppose we arrange that it shall not do so, but, instead of a curve, stretch out and constrain the surface to follow in a straight line of plane, directed from the angle or leading portion of the front edge most effective for sustaining weight, would the terminal part of the flat surface be effective for a like purpose?

"Quite the reverse. It would actually be *dragged down* by the currents of air deflected from the first part or edge, so that if this were to be set *per se* at the best incline for support and minimum resistance, the descending air or following portion would compel the front edge to take a more obtuse angle, improper for the least amount of forward resistance. In order to meet the varying directions of incompatible deflected currents, the whole surface has to take an average inclination exceeding the lowest degree at which a narrower surface (having in consequence a diminished resistance) would require to be set.

"If such a form is given to the following portion of the parabolic surface as to derive some abutment against the receding air that had been previously deflected from a leading portion, the final part is but of little use for sustaining any weight, because it is wastefully *acting against a yielding support on air that has before been carried away from it and so used up.* After the first impact, and the air begins to recede, the sooner that is got rid of the better. Let all the weight be sustained by *first impact* as far as possible.

"Referring again to this parabolic curve: Supposing that we copy the figure and give the same outline to a thin sheet of metal fixed in like form and position. This has not altered the conditions. The effect as regards the action of the air remains similar for the same velocity of currents; then let the metal be cut through in numerous equidistant strips and all of them turned back at the same angle from the horizontal, as shown by the dotted lines at the back of the curve. We have thus cut up and destroyed the surface so that the air *rushes through it* in parallel strata. Instead of loss of effect, the *gain* under these circumstances is remarkable as merely arising from this apparent disunion of surface into fragmentary portions. The bulky body of air that was turned down so as to be useless near its final exit now passes through in straight lines with very little deflection, each stratum giving up the force of first impact and maximum lifting effect before it eventually leaves the interstices of the *louvres*.

"The tangent turbine is one of the most effective forms of water motor; it consists of an extended rotating ring of thin, narrow blades, somewhat hollow or curved



Wenham's parabolic curve.

in cross outline, which rotates round a current of water radiating from the axis of the machine. The impact of the first portion only of the water on the narrow blades is taken as serviceable; the residual force of the spent or "dead water" is not worth consideration.

"When a narrow blade inclined upward is propelled at a high speed through the air there is no doubt a diminution of pressure on the top surfaces, which tends to increase the lifting effect; all thickness of substance, however placed, given on the upper side of the plane, will diminish this ascential force. The front edge of the wing of a bird is thick, as a matter of necessity in natural construction, and in no way aids the faculty of flight.

"Among other experiments by the writer, a system of aeroplanes was made out of the thinnest tin plate that could be procured—·005 in. thick and weighing 3 oz. to the foot. The blades were concave beneath. The lifting power of this arrangement exceeded anything before tried, and conveyed the idea that the best effort would be obtained if the supporting surfaces were to be made of smooth metal. Strips of aluminium might be used, for the sake of lightness; the proper surface curve to be given by passing them lengthwise through rollers turned to a similar curve."

In this paper Wenham considered the multiplane construction and the particular advantages to be obtained by giving the planes a backward or forward stagger.

For experimental work he strongly advocated the use of kites although he admitted he had not obtained any data worth recording in some experiments which he had tried. He urged that data must be obtained by experiment for the best form of airscrews and after some comment on the general principles to be followed in airscrew design he finishes, "All the so-termed screws which I have seen as applied to flying models and machines have been mere flat plates fixed on to radial arms." He expressed his "conviction that form in both supporting surfaces and also in those for propulsion is of the utmost importance in aerial support and progression."

Wenham emphasised his belief, derived from experiment, that not only was the leading edge important in flight, but it was of great importance to have smooth

surfaces both for the wings and airscrews. His dictum about the importance of smooth surfaces drew a comment from Professor Todd of Amherst College, who had himself been carrying out experiments.

"Mr. Wenham's suggestions appear to me to be of the utmost significance . . . . The importance of smooth surfaces cannot be too prominently kept in view. In some of the most successful experiments I have made with models, strips of thin glass formed the aeroplanes."

Chanute, in his book on Flying Machines, pays high tribute to Wenham's work, but points out that Wenham's multiplanes were "even more unstable than that of a bird," and that it was necessary "to seek some form of apparatus which shall be, if possible, more stable in equilibrium than the bird."

But Wenham was well aware of the difficulties lying ahead. In September 1897, in a letter on The Aerial Problem, written to the *English Mechanic*, he said:

"The development of a successful flying machine must be from the result of careful tentative preliminary experiments, and any one at once constructing a flying machine without a study of the principles and a knowledge of what has been done before, and learned the cause of failure, will certainly not succeed."

Perhaps Wenham's outlook on the general problem of flight is best seen in an article he wrote in 1903 in the American publication *The Aeronautical World*.

"I have always avoided adverse criticisms of designs provided with sustaining wings, as most of the knowledge we possess has been the result of failures," he began. "I am also disinclined to give mere suggestions of my own that have not been the outcome of actual experiment."

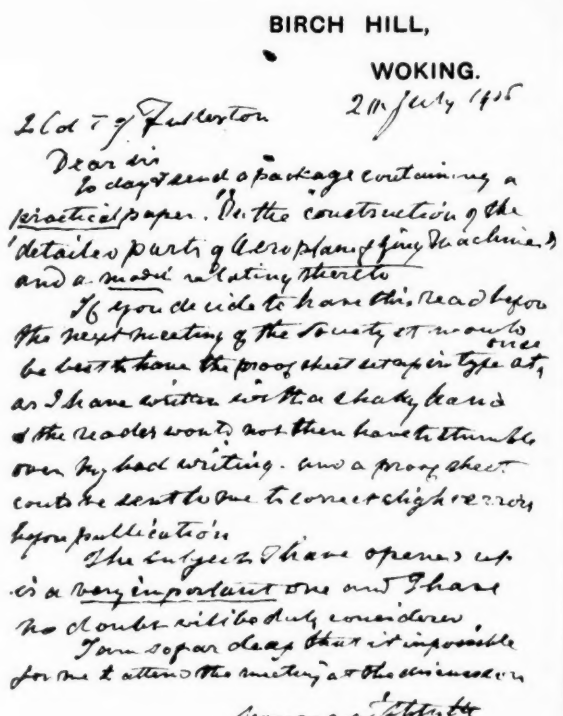
He describes his methods for designing and making airscrews and the experiments he had made from time to time.

"Not any of the experimental screws I have made exceeded 24 inches in diameter; they were all cast from wooden patterns; the true surface was carved by a rotating drill cutter to which the screw was guided on a sliding axle rod against a circumferential and radial template."

He tested these screws at "1,000 revolutions per minute (more or less)." He discusses much about flight which has already been noted in this paper.

"But a word about the so-termed aeroplanes. Flight is impossible with flat supporting surfaces, as nearly all the lift is confined to a narrow portion of the front edge . . . . I still believe mechanical flight to be practicable with very much less power than has been stated. Gliding flights have not gone much further than to solve the problem of equilibrium; a flying machine must fly on the level. When descent begins, acceleration occurs, and it is not an easy matter to rise against this accumulated force.

"It may be asked why I have not carried these ideas into practice myself? In experimental machines I have been in the habit of constructing the greatest part of them with my own hand. This is fatiguing work, as I am now in my eightieth year, and would prefer to be an observer of acrobatic performances rather than engage in them personally."



BIRCH HILL,

WOKING.

21st July 1908

Lt Col J. Fullerton

Dear Sir

To day I send a package containing a practical paper "On the construction of the detailed parts of Aeroplanes" and a model relating thereto

If you decide to have this read before the next meeting of the Society it would be best to have the proof sheet set up in type at once as I have written with a shaky hand & the reader would not then have to tumble over my bad writing—and a proof sheet could be sent to me to correct slight errors before publication.

The subject I have opened up is a very important one and I have no doubt will be duly considered.

I am so far deaf that it is impossible for me to attend the meeting at the discussion.

Yours faithfully

F. H. Wenham

Wenham's last letter to the Society:

Lt.-Col. J. D. Fullerton. 20 July 1908.

Dear Sir,

To day I send a package containing a practical paper "On the construction of the detailed parts of aeroplanes" and a model relating thereto. If you decide to have this read before the next meeting of the Society it would be best to have the proof sheet set up in type at once as I have written with a shaky hand and the reader would not then have to tumble over my bad writing—and a proof sheet could be sent to me to correct slight errors before publication. The subject I have opened up is a very important one and I have no doubt will be duly considered. I am so far deaf that it is impossible for me to attend the meeting at the discussion.

Yours faithfully, F. H. WENHAM.

(He died three weeks later, in his 85th year)

In discussing the position of the airscrew in an aircraft, Wenham wrote, "In an experiment I should place the screw vanes in front . . . . in a line with the supporting air surface. Any backward current would then meet the incline of the planes, tending to raise the weight. I do not assert that such would be the case; but as yet experiments must be tentative, and it would be preferable to have the screw in front . . . . I have found the most satisfactory deductions from models placed in a current of air of known velocity from a fan blower. I have some further experiments in view which I will try, as every condition is so clearly under observation and measurable."

Wenham was born in 1824 and still going strong!

In July 1908 Wenham wrote a note for the *Aeronautical Journal* on a point of longitudinal stability, and described a new experiment he had been trying with

a fan blower, as "a screw working in air gives off such a feeble reaction that other means of propulsion should be sought for." His experiment was aimed at getting more power out of the airscrew in a different way, but he admitted it had been "somewhat indecisive" although "the effect was sufficient to warrant a trial on a larger scale."

The following month, on 11th August 1908, Francis Herbert Wenham died at Folkestone, where he had gone to recuperate. Only a few days before his death he had completed his last article, "On the construction of detailed parts of aeroplane flying machines." It was published in the October 1908 issue of the *Aeronautical Journal* immediately after the brief notice of his death.

In this last paper he said "For many years I have at times brought up the subject of flying, and if I say a few words concerning myself I may, perhaps, be exempt from incurring a charge of egotism."

In those few words about himself he said, "It may be asked why I did not apply a screw propeller to my design of a flying machine in 1866, but no motive power except steam was then known, and I could not imagine the practicability of carrying a supply of water and fuel up in the air, and therefore had to consider the possibility of a man being sufficient to propel a machine .... I am still inclined to believe that an active man of light weight could fly short distances by his own muscular exertion ...."

Wenham's words are not very far off becoming a reality, for the study of aerodynamics has progressed so greatly in the past fifty years that the distinguished chief engineer of British European Airways has been able to point the way for man-power flight.

Only three days before his death Wenham wrote to V. S. Frank, a member of the Society then working in Paris.

"I have received the photographs you sent me of flying machines," he replied. "On this subject I have made it a rule to avoid criticism on the work of others; but your photographs conveyed to my mind that most of them were *unnecessarily clumsy and heavy*, and might be cut down and lightened without diminution or sacrificing strength."

This Memoir ends with an extract from the Minutes of the Council of the Aeronautical Society of Great Britain, of their meeting of 8th March 1910.

"7. Wenham Medal

A proposal was introduced to institute an annual award of a medal for the most distinguished service rendered to Aeronautical Science during each year by a British subject, such medal to be distinct from those already instituted by the Society, and to be called the "Wenham." The assistant Secretary was directed to bring the proposal before the next Council meeting for further consideration."

But there is no further reference in the Council Minutes.

I am very much indebted to the staffs of the libraries of the Royal Aeronautical Society and the Royal Microscopical Society for their assistance in the research inquiries I had to make. I wish to pay a special tribute to J. W. Kettlewell, Assistant Secretary of the R.M.S., for his personal help.

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EARLY DAYS

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The first meeting of the members of this Society was held at the great room of the Society of Arts, on the 27th day of June 1866. His Grace the Duke of Argyll in the chair.

The following paper, *On Aerial Locomotion and the Laws by which Heavy Bodies impelled through Air are Sustained*, was read by F. H. Wenham, Esq.

The resistance against a surface of a defined area, passing rapidly through yielding media, may be divided into two opposing forces. One arising from the cohesion of the separated particles; and the other from their weight and inertia, which, according to well-known laws, will require a constant power to set them in motion.

In plastic substances, the first condition, that of cohesion, will give rise to the greatest resistance. In water this has very little retarding effect, but in air, from its extreme fluidity, the cohesive force becomes inappreciable, and all resistances are caused by its weight alone; therefore, a weight, suspended from a plane surface, descending perpendicularly in air, is limited in its rate of fall by the weight of air that can be set in motion in a given time.

If a weight of 150 lbs. is suspended from a surface of the same number of square feet, the uniform descent will be 1,300 feet per minute, and the force given out and expended on the air, at this rate of fall, will be nearly six horse-power; and, conversely, this same speed and power must be communicated to the surface to keep the weight sustained at a fixed altitude. As the surface is increased, so does the rate of descent and its accompanying power, expended in a given time, decrease. It might, therefore, be inferred that, with a sufficient extent of surface reproduced, or worked up to a higher altitude, a man might

by his exertions raise himself for a time, while the surface descends at a less speed.

A man, in raising his own body, can perform 4,250 units of work—that is, this number of pounds raised one foot high per minute—and can raise his own weight—say, 150 lbs.—twenty-two feet per minute. But at this speed the atmospheric resistance is so small that 120,000 square feet would be required to balance his exertions, making no allowance for weight beyond his own body.

We have thus reasons for the failure of the many misdirected attempts that have, from time to time, been made to raise weights perpendicularly in the air by wings or descending surfaces. Though the flight of a bird is maintained by a constant reaction or abutment against an enormous weight of air in comparison with the weight of its own body, yet, as will be subsequently shown, the support upon that weight is not necessarily commanded by great extent of wing-surface, but by the direction of motion.

One of the first birds in the scale of flying magnitude is the pelican. It is seen in the streams and estuaries of warm climates, fish being its only food. On the Nile, after the inundation, it arrives in flocks of many hundreds together, having migrated from long distances. A specimen shot was found to weigh twenty-one pounds, and measured ten feet across the wings, from end to end. The pelican rises with much difficulty, but, once on the wing, appears to fly with very little exertion, notwithstanding its great weight. Their mode of progress is peculiar and graceful. They fly after a leader, in one single train. As he rises or descends, so his followers do the same in succession, imitating his movements precisely. At a distance, this gives them the appearance of a long undulating ribbon, glistening under the cloudless sun of an oriental sky. During their flight they make about seventy strokes per minute with their wings. This uncouth-looking bird is somewhat whimsical in its habits. Groups of them may be seen far above the earth, at a distance from the river-side, *soaring*, apparently for their own pleasure. With outstretched and motionless wings, they float serenely, high in the atmosphere, for more than an hour together, traversing the same locality in circling movements. With head thrown back, and enormous bills resting on their breasts, they almost seem asleep. A few easy strokes of their wings each minute, as their momentum or velocity diminishes, serves to keep them sustained at the same level. The effort required is obviously slight, and not confirmatory of the excessive amount of power said to be requisite for maintaining the flight of a bird of this weight and size. The pelican displays no symptom of being endowed with great strength, for when only slightly wounded it is easily captured, not having adequate power for effective resistance, but heavily flapping the huge wings, that should, as some imagine, give a stroke equal in vigour to the kick of a horse.

During a calm evening, flocks of spoonbills take their flight directly up the river's course; as if linked together in unison, and moved by the same impulse, they alter not their relative positions, but at less than fifteen inches above the water's surface, they speed swiftly by with ease and grace inimitable, a living sheet of spotless white. Let one circumstance be remarked—though they have fleeted past at a rate of near thirty miles an hour, so little do they disturb the element in which they move, that not a ripple of the placid bosom of the river, which they almost touch, has marked their track. How wonderfully does their progress contrast with that of creatures who are compelled to drag their slow and weary way against the fluid a thousandfold more dense, flowing in strong and eddying current beneath them.

Our pennant droops listlessly, the wished-for north wind cometh not. According to custom we step on shore, gun in hand. A flock of white herons, or " buffalo-birds," almost within our reach, run a short distance from the pathway as we approach them. Others are seen perched in social groups upon the backs of the apathetic and mud-begrimed animals whose name they bear. Beyond the ripening dhourra crops which skirt the river-side, the land is covered with immense numbers of blue pigeons, flying to and fro in shoals, and searching for food with restless diligence. The musical whistle from the pinions of the wood-doves sounds cheerily, as they dart past with the speed of an arrow. Ever and anon are seen a covey of the brilliant, many-coloured partridges of the district, whose *long and pointed wings* give them a strength and duration of flight that seems interminable, alighting at distances beyond the possibility of marking them down, as we are accustomed to do with their plumper brethren at home. But still more remarkable is the spectacle which the sky presents. As far as the eye can reach it is dotted with birds of prey of every size and description. Eagles, vultures, kites and hawks, of manifold species, down to the small, swallow-like, insectivorous hawk common in the Delta, which skims the surface of the ground in pursuit of its insect prey. None seem bent on going forward, but all are soaring leisurely round over the same locality, as if the invisible element which supports them were their medium of rest as well as motion. But mark that object sitting in solitary state in the midst of yon plain: what a magnificent eagle! An approach to within eighty yards arouses the king of birds from his apathy. He partly opens his enormous wings, but stirs not yet from his station. On gaining a few feet more he begins to *walk* away, with half-expanded but motionless wings. Now for the chance fire! A charge of No. 3 from 11 bore rattles audibly but ineffectively upon his densely feathered body; his walk increases to a run, he gathers speed with his slowly-waving wings, and eventually leaves the ground. Rising at a gradual inclination, he mounts aloft and sails majestically away to his place of refuge in the Lybian range, distant at least five miles from where he rose. Some fragments of feathers denote the spot where the shot had struck him. The marks of his claws are traceable in the sandy soil, as, at first with firm and decided digs, he forced his way, but as he lightened his body and increased his speed with the aid of his wings, the imprints of his talons gradually merged into long scratches. The measured distance from the point where these vanished, to the place where he had stood, proved that with all the stimulus that the shot must have given to his exertions, he had been compelled to run full twenty yards before he could raise himself from the earth.

Again the boat is under weigh, though the wind is but just sufficient to enable us to stem the current. An immense kite is soaring overhead, scarcely higher than the top of our lateen yard, affording a fine opportunity for contemplating his easy and unlaboured movements. The cook has now thrown overboard some offal. With a solemn swoop the bird descends and seizes it in his talons. How easily he rises again with motionless expanded wings, the mere force and momentum of his *descent* serving to raise him again to more than half-mast high. Observe him next, with lazy flapping wings, and head turned under his body; he is placidly devouring the pendant morsel from his foot, and calmly gliding onwards.

The Nile abounds with large aquatic birds of almost every variety. During a residence upon its surface for nine months out of the year, immense numbers have been seen to come and go, for the majority of them are migratory. Egypt being merely a narrow strip of territory, passing through one of the most desert parts of the earth,

and rendered fertile only by the periodical rise of the waters of the river, it is probable that these birds make it their grand thoroughfare into the rich districts of Central Africa.

On nearing our own shores, steaming against a moderate head-wind, from a station abaft the wheel the movements of some half-dozen gulls are observed, following in the wake of the ship, in patient expectation of any edibles that may be thrown overboard. One that is more familiar than the rest come so near at times that the winnowing of his wings can be heard; he has just dropped astern, and now comes on again. With the axis of his body exactly at the level of the eyesight, his every movement can be distinctly marked. He approaches to within ten yards, and utters his wild plaintive note, as he turns his head from side to side, and regards us with his jet black eye. But where is the angle or upward rise of his wings, that should compensate for his descending tendency, in a yielding medium like air? The incline cannot be detected, for, to all appearance, his wings are edgewise, or parallel to his line of motion, and he appears to skim along a *solid* support. No smooth-edged rails, or steel-tired wheels, with polished axles revolving in well oiled brasses, are needed here for the purpose of diminishing friction, for Nature's machinery has surpassed them all. The retarding effects of gravity in the creature under notice, are almost annulled, for he is gliding forward upon a *frictionless* plane. There are various reasons for concluding that the direct flight of many birds is maintained with a much less expenditure of power, for a high speed, than by any mode of progression.

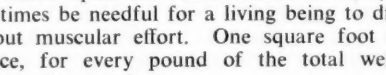
The first subject for consideration is the proportion of surface to weight, and their combined effect in descending perpendicularly through the atmosphere. The datum is here based upon the consideration of *safety*, for it may sometimes be needful for a living being to drop passively, without muscular effort. One square foot of sustaining surface, for every pound of the total weight, will be sufficient for security.

According to Smeaton's table of atmospheric resistances, to produce a force of *one pound* on a square foot, the wind must move against the plane (or, which is the same thing, the plane against the wind), at the rate of twenty-two feet per second, or 1,320 feet per minute, equal to fifteen miles per hour. The resistance of the air will now balance the weight of the descending surface, and, consequently, it cannot exceed that speed. Now, twenty-two feet per second is the velocity acquired at the *end* of a fall of eight feet—a height from which a well-knit man or animal may leap down without much risk of injury. Therefore, if a man with parachute weigh together 143 lbs., spreading the same number of square feet of surface contained in a circle fourteen-and-a-half feet in diameter, he will descend at perhaps an unpleasant velocity, but with safety to life and limb.

It is a remarkable fact how this proportion of wing-surface to weight extends throughout a great variety of the flying portion of the animal kingdom, even down to hornets, bees, and other insects. In some instances, however, as in the gallinaceous tribe, including pheasants, this area is somewhat exceeded, but they are known to be very poor flyers. Residing as they do chiefly on the ground, their wings are only required for short distances, or for raising them or easing their descent from their roosting-places in forest trees, the *shortness* of their wings preventing them from taking extended flights. The wing-surface of the common swallow is rather more than in the ratio of *two* square feet per pound, but having also great length of pinion, it is both swift and enduring in its flight. When on a rapid course this bird is in the habit of furling its wings into a narrow compass. The greater extent of surface is probably needful for the continual variations of speed and instant stoppages requisite for obtaining its insect food.

On the other hand, there are some birds, particularly of the duck tribe, whose wing-surface but little exceeds *half* a square foot, or seventy-two inches per pound, yet they may be classed among the strongest and swiftest of flyers. A weight of one pound, suspended from an area of this extent, would acquire a velocity due to a fall of sixteen feet—a height sufficient for the destruction or injury of most animals. But when the plane is urged forward horizontally, in a manner analogous to the wings of a bird during flight, the sustaining power is greatly influenced by *the form and arrangement* of the surface.

In the case of *perpendicular* descent, as a parachute, the sustaining effect will be much the same, whatever the figure of the outline of the superficies may be, and a circle perhaps affords the best resistance of any. Take for example a circle of twenty square feet (as possessed by the pelican) loaded with as many pounds. This, as just stated, will limit the rate of perpendicular descent to 1,320 feet per minute. But instead of a circle sixty-one inches in diameter, if the area is bounded by a parallelogram ten feet long by two feet broad, and whilst at perfect freedom to descend perpendicularly, let a force be applied exactly in a horizontal direction, so as to carry it edgeways, with the long side foremost, as a forward speed of thirty miles per hour—just double that of its passive descent: the rate of fall under these conditions will be decreased most remarkably, probably to less than one fifteenth part, or eighty-eight feet per minute, or one mile per hour.

The annexed line represents transversely the plane two feet wide and ten feet long, moving in the direction of the arrow 

with a forward speed of thirty miles per hour, or 2,640 feet per minute, and descending at eighty-eight feet per minute, the ratio being as one to thirty. Now, the particles of air, caught by the forward edge of the plane, must be carried down eight-tenths of an inch before they leave it. This stratum, ten feet wide and 2,640 long, will weigh not less than 134 lbs.; therefore the weight has continually to be moved downwards, eighty-eight feet per minute, from a state of absolute rest. If the plane, with this weight and an upward rise of eight-tenths of an inch, be carried forward at a rate of thirty miles per hour, it will be maintained at the same level without descending.

The following illustration, though referring to the action of surfaces in a denser fluid, are yet exactly analogous to the conditions set forth in air:—

Take a stiff rod of wood, and nail to its end at right angles a thin lath or blade, about two inches wide. Place the rod square across the thwarts of a rowing-boat in motion, letting a foot or more of the blade hang perpendicularly over the side into the water. The direct amount of resistance of the current against the flat side of the blade may thus be felt. Next slide the rod to and fro thwart ship, keeping all square; the resistance will now be found to have increased enormously; indeed, the boat can be entirely stopped by such an appliance. Of course the same experiment may be tried in a running stream.

Another familiar example may be cited in the leeboards and sliding keels used in vessels of shallow draught, *which act precisely on the same principle as the plane or wing-surface of a bird when moving in air.* These surfaces, though parallel to the line of the vessel's course, enable her to

carry a heavy press of sail without giving way under the side pressure, or making lee-way, so great is their resistance against the rapidly passing body of water, which cannot be deflected sideways at a high speed.

The succeeding experiments will serve further to exemplify the action of the same principle. Fix a thin blade, say one inch wide and one foot long, with its plane exactly midway and at right angles, to the end of a spindle or rod. On thrusting this through a body of water, or immersing it in a stream running in the direction of the axis of the spindle, the resistance will be simply that caused by the water against the mere superficies of the blade. Next put the spindle and blade in rapid rotation. The retarding effect against direct motion will now be increased near *ten-fold* and is equal to that due *to the entire area of the circle of revolution*. By trying the effect of blades of various widths, it will be found that, for the purpose of effecting the maximum amount of resistance, the more rapidly the spindle revolves the narrower may be the blade. There is a specific ratio between the *width* of the blade and its *velocity*. It is of some importance that this should be precisely defined, not only for its practical utility in determining the best proportion of width to speed in the blades of screw-propellers, but also for a correct demonstration of the principles involved in the subject now under consideration; for it may be remarked that the swiftest-flying birds possess extremely long and *narrow* wings, and the slow, heavy flyers short and wide ones.

In the early days of the screw-propeller, it was thought requisite, in order to obtain the advantage of the utmost extent of surface, that the end-view of the screw should present no opening, but appear as a complete disc. Accordingly, some were constructed with one or two threads, making an entire or two half-revolutions; but this was subsequently found to be a mistake. In the case of the two blades, the length of the screw was shortened, and consequently the width of the blades reduced, with increased effect, till each was brought down to considerably less than *one-sixth* of the circumference or area of the entire circle; the maximum speed was then obtained. Experiment has also shown that the effective propelling area of the two-bladed screw is tantamount to its entire circle of revolution, and is generally estimated as such.

Many experiments tried by the author, with various forms of screws, applied to a small steam-boat, led to the same conclusion—that the two blades of one-sixth of the circle gave the best result.

All screws reacting on a fluid such as water, must cause it to yield to some extent; this is technically known as "slip," and whatever the ratio or per-centage on the speed of the boat may be, it is tantamount to *just so much loss of propelling power*—this being consumed in giving motion to the water instead of the boat.

On starting the engine of the steam-boat referred to, and grasping a mooring-rope at the stern, it was an easy matter to hold it back with one hand, though the engine was equal in power to five horses, and the screw making more than 500 revolutions per minute. The whole force of the steam was absorbed in "slip," or in giving motion to the column of water; but let her go, and allow the screw to find an abutment on a fresh body of water, not having received a gradual motion, and with its *inertia undisturbed* when running under full way, the screw worked almost as if in a solid nut, the "slip" amounting to only 11 per cent.

The laws which control the action of inclined surfaces, moving either in straight lines or circles in *air*, are identical, and serve to show the inutility of attempting to raise a heavy body in the atmosphere by means of rotating vanes or a screw acting vertically; for unless the ratio of surface compared to weight is exceedingly extensive, the whole power will be consumed in "slip," or in giving a downward motion to the column of air. Even if a sufficient force is obtained to keep a body suspended by such means, yet, after the desired altitude is arrived at, *no further ascension* is required; there the apparatus is to remain stationary as to level, and its position on the constantly yielding support can only be maintained at an enormous expenditure of power, for the screw cannot obtain a hold upon a *fresh and unmoved* portion of air in the same manner as it does upon the body of water when propelling the boat at full speed; its action under these conditions is the same as when the boat is held fast, in which case, although the engine is working up to its usual rate, the tractive power is almost annulled.

Some experiments made with a screw, or pair of inclined vanes acting vertically in air, were tried, in the following manner. To an upright post was fixed a frame, containing a bevel wheel and pinion, multiplying in the ratio of three to one. The axle of the wheel was horizontal, and turned by a handle of five-and-a-half inches radius. The spindle of the pinion rotated vertically, and carried two driving-pins at the end of a cross-piece, so that the top resembled the three prongs of a trident. The upright shaft of the screw was bored hollow to receive the middle prong, while the two outside ones took a bearing against a driving-bar, at right angles to the lower end of the shaft, the top of which ended in a long iron pivot, running in a socket fixed in a beam overhead; it could thus rise and fall about two inches with very little friction. The top of the screw-shaft carried a cross-arm, with a blade of equal size at each extremity, the distance from end to end being six feet. The blades could be adjusted at any angle by clamping-screws. Both their edges, and the arms that carried them, were bevelled away to a sharp edge to diminish the effects of atmospheric resistance. A wire stay was taken from the base of each blade to the bottom of the upright shaft, to give rigidity to the arms, and to prevent them from springing upwards. With this apparatus experiments were made with weights attached to the upright screw-shaft, and the blades set at different pitches, or angles of inclination. When the vanes were rotated rapidly, they rose and floated on the air, carrying the weights with them. Much difficulty was experienced in raising a heavy weight by a comparatively small extent of surface, moving at a high velocity; the "slip" in these cases being so great as to absorb all the power employed. The utmost effect obtained in this way was to raise a weight of six pounds on one square foot of sustaining surface, the planes having been set at a coarse pitch. To keep up the rotation, required about half the power a man could exert.

The ratio of weight to sustaining surface was next arranged in the proportion approximating to that of birds. Two of the experiments are here quoted, which gave the most satisfactory result. Weight of wings and shaft, 17½ oz.; area of two wings, 121 inches—equal to 110 square inches per pound. The annexed figures are given approximately, in order to avoid decimal fractions:—

	No. of revolutions per minute	*Mean sustaining speed. Miles per hour*	*Feet per minute*	*Pitch or angle of rise in one revolution. Inches*	*Ratio of pitch to speed*	*Slip per cent.*
1st Experiment	210	38	3,360	26	⅛th nearly	12½
2nd do.	240	44	3,840	15	1/13th do.	8

The power required to drive was nearly the same in both experiments—about equal to one-sixteenth part of

a horse-power, or the third part of the strength of a man, as estimated by a constant force on the handle of twelve pounds in the first experiment, and ten in the second, the radius of the handle being five-and-a-half inches, and making seventy revolutions per minute in the first case, and eighty in the other.

These experiments are so far satisfactory in showing the small pitch or angle of rise required for sustaining the weight stated, and demonstrating the principle before alluded to, of the slow descent of planes moving horizontally in the atmosphere at high velocities; but the question remains to be answered, concerning the disposal of the excessive power consumed in raising a weight not exceeding that of a carrier pigeon, for unless this can be satisfactorily accounted for, there is but little prospect of finding an available power, of sufficient energy in its application to the mechanism, for raising apparatus, either experimental or otherwise, in the atmosphere. In the second experiment, the screw-shaft made 240 revolutions, consequently, one vane (there being two) was constantly passing over the *same spot* 480 times each minute, or eight times in a second. This caused a descending current of air, moving at the rate of near four miles per hour, almost sufficient to blow a candle out placed three feet underneath. This is the result of "slip," and the giving both a downward and rotary motion to this column of air, will account for a great part of the power employed, as the whole apparatus performed the work of a blower. If the wings, instead of travelling in a circle, could have been urged continually forward in a straight line in a fresh and unmoved body of air, the "slip" would have been so inconsiderable, and the pitch, consequently, reduced to such a small angle, as to add but little to the direct forward atmospheric resistance of the edge.

The small flying screws, sold as toys, are well known. It is an easy matter to determine approximately the force expended in raising and maintaining them in the atmosphere. The following is an example of one constructed of tin-plate with three equidistant vanes. This was spun by means of a cord, wound round a wooden spindle, fitted into a forked handle as usual. The outer end of the coiled string was attached to a small spring steelyard, which served as a handle to pull it out by. The weight, or degree at which the index had been drawn, was *afterwards* ascertained by the mark left thereon by a pointed brass wire. It is not necessary to know the *time* occupied in drawing out the string, as this item in the estimate may be taken as the duration of the ascent; for it is evident that if the same force is re-applied at the descent, it would rise again, and a repeated series of these impulses will represent the power required to prolong the flight of the instrument. It is, therefore, requisite to know the length of string, and the force applied in pulling it out. The following are the data:—

Diameter of screw - -	8½ inches.
Weight of ditto - - -	396 grains.
Length of string drawn out -	2 feet.
Force employed - - -	8 lbs.
Duration of flight - -	16 seconds.

From this it may be computed that, in order to maintain the flight of the instrument, a constant force is required of near sixty foot-pounds per minute—in the ratio of about three horse-power for each hundred pounds raised by such means. The force is perhaps over-estimated for a larger screw, for as the size and weight is increased, the power required would be less than in this ratio. The result would be more satisfactory if tried with a sheet-iron screw, impelled by a descending weight

Method analogous to this have been proposed for attempting aerial locomotion; but experiment has shown that a screw rotating in the air is an imperfect principle for obtaining the means of flight, and supporting the needful weight, for the power required is enormous. Suppose a machine to be constructed, having some adequate supply of force, the screw rotating vertically at a certain velocity will raise the whole. When the desired altitude is obtained, nearly the same velocity of revolution, and the same excessive power, must be continued, and consumed *entirely in "slip,"* or in drawing down a rapid current of air.

If the axis of the screw is slightly inclined from the perpendicular, the whole machine will travel forward. The "slip," and consequently the power, is somewhat reduced under these conditions; but a swift forward speed cannot be affected by such means, for the resistance of the inclined disc of the screw will be very great, far exceeding any form assimilating to the edge of the wing of a bird. But, arguing on the supposition that a forward speed of thirty miles an hour might thus be obtained, even then nearly all the power would be expended in giving an unnecessary and rapid revolution to an immense screw, capable of raising a weight, say of 200 pounds. The weight alone of such a machine must cause it to fail, and every revolution of the screw is a subtraction from the much-desired direct forward speed. A simple narrow blade, or inclined plane, propelled in a direct course at *this* speed—which is amply sufficient for sustaining heavy weights—is the best, and, in fact, the only means of giving the maximum amount of supporting power with the least possible degree of "slip," and direct forward resistance. Thousands of examples in Nature testify its success, and show the principle in perfection;—apparently the only one, and therefore beyond the reach of amendment, the wing of a bird, combining a propelling and supporting organ in one, each perfectly efficient in its mechanical action.

This leads to the consideration of the amount of power requisite to maintain the flight of a bird. Anatomists state that the pectoral muscles for giving motion to the wings are excessively large and strong; but this furnishes no proof of the expenditure of a great amount of force in the act of flying. The wings are hinged to the body like two powerful levers, and some counteracting force of a *passive* nature, acting like a spring under tension, must be requisite merely to balance the weight of the bird. It cannot be shown that, while there is no active motion, there is any real exertion of muscular force; for instance, during the time when a bird is soaring with motionless wings. This must be considered as a state of equilibrium, the downward spring and elasticity of the wings serving to support the body; the muscles, in such a case, performing like stretched india-rubber springs would do. The motion or active power required for the performance of flight must be considered exclusive of this.

It is difficult, if not impossible, by any form of dynamometer, to ascertain the precise amount of force given out by the wings of birds; but this is perhaps not requisite in proof of the principle involved, for when the laws governing their movements in air are better understood, it is quite possible to demonstrate, by isolated experiments, the amount of power required to sustain and propel a given weight and surface at any speed.

If the pelican referred to as weighing twenty-one pounds, with near the same amount of wing-area in square feet, were to descend perpendicularly, it would fall at the rate of 1,320 feet per minute, being limited to this speed by the resistance of the atmosphere.

The standard generally employed in estimating power is by the rate of descent of a weight. Therefore, the weight

of the bird being twenty-one pounds, which, falling at the above speed will expend a force on the air set in motion nearly equal to one horse (·84 h.p.) or that of *five* men; and conversely, to raise this weight again perpendicularly upon a yielding support like air, would require even more power than this expression, which it is certain that a pelican does not possess; nor does it appear that any *large* bird has the faculty of raising itself on the wing *perpendicularly* in a still atmosphere. A pigeon is able to accomplish this nearly, mounting to the top of a house in a very narrow compass; but the exertion is evidently severe, and can only be maintained for a short period. For its size, this bird has great power of wing; but this is perhaps far exceeded in the humming-bird, which, by the extremely rapid movements of its pinions, sustains itself for more than a minute in still air in one position. The muscular force required for this feat is much greater than for any other performance of flight. The body of the bird at the time is nearly vertical. The wings uphold the weight, not by striking vertically downwards upon the air, but as inclined surfaces reciprocating horizontally like a screw, but wanting in its continuous rotation in one direction, and, in consequence of the loss arising from rapid alternations of motion, the power required for the flight will exceed that specified in the screw experiment before quoted, *viz.*: three horse-power for every 100 pounds raised.

We have here an example of the exertion of enormous animal force expended in flight, necessary for the peculiar habits of the bird, and for obtaining its food; but in the other extreme, in large heavy birds, whose wings are merely required for the purposes of migration or locomotion, flight is obtained with the least possible degree of power, and this condition can only be commanded by a rapid straightforward course through the air.

The sustaining power obtained in flight must depend upon certain laws of action and reaction between relative weights; the weight of a bird, balanced, or finding an abutment, against the fixed inertia of a far greater weight of air, continuously brought into action in a given time. This condition is secured, not by extensive surface, but by great length of wing, which, in forward motion, takes a support upon a wide stratum of air, extending transversely to the line of direction.

The pelican, for example, has wings extending out ten feet. If the limits of motion imparted to the substratum of air, acted upon by the incline of the wing, be assumed as one foot in thickness, and the velocity of flight as thirty miles per hour, or 2,640 feet per minute, the stratum of air passed over in this time will weigh nearly one ton, or one hundred times the weight of the body of the bird, thus giving such an enormous supporting power, that the comparatively small weight of the bird has but little effect in deflecting the heavy length of stratum downwards, and, therefore, the higher the velocity of flight the less the amount of " slip," or power wasted in compensation for descent.

As noticed at the commencement of this paper, large birds may be observed to skim close above smooth water without ruffling the surface; showing that during rapid flight the air does not give way beneath them, but approximates towards a solid support.

In all inclined surfaces, moving rapidly through air, the whole sustaining power approaches toward the front edge; and in order to examplify the inutility of surface alone, without proportionate length of wing, take a plane, ten feet long by two broad, impelled with the narrow end forward, the first twelve or fifteen inches will be as efficient at a high speed in supporting a weight as the entire following portion of the plane, which may be cut off, thus reducing the effective wing-area of a pelican, arranged in this direction, to the totally inadequate equivalent of two-and-a-half square feet.

One of the most perfect natural examples of easy and long-sustained flight is the wandering albatross. " A bird for endurance of flight probably unrivalled. Found over all parts of the Southern Ocean, it seldom rests on the water. During storms, even the most terrific, it is seen now dashing through the whirling clouds, and now serenely floating, without the least observable motion of its outstretched pinions." The wings of this bird extend fourteen or fifteen feet from end to end, and measure only eight-and-a-half inches across the broadest part. This conformation gives the bird such an extraordinary sustaining power, that it is said to *sleep* on the wing during stormy weather, when rest on the ocean is impossible. Rising high in the air, it skims slowly down, with absolutely motionless wings, till a near approach to the waves awakens it, when it rises again for another rest.

If the force expended in actually sustaining a long-winged bird upon a wide and unyielding stratum of air, during rapid flight, is but a small fracton of its strength, then nearly the whole is exerted in overcoming direct forward resistance. In the pelican referred to, the area of the body, at its greatest diameter, is about 100 square inches; that of the pinions, eighty. But as the contour of many birds during flight approximates nearly to Newton's solid of least resistance, by reason of this form, acting like the sharp bows of a ship, the opposing force against the wind must be reduced down to one-third or fourth part; this gives one-tenth of a horse-power, or about half the strength of a man, expended during a flight of thirty miles per hour. Judging from the action of the living bird when captured, it does not appear to be more powerful than here stated.

The transverse area of a carrier pigeon during flight (including the outstretched wings) a little exceeds the ratio of twelve square inches for each pound, and the wing-surface, or sustaining area, ninety square inches per pound.

Experiments have been made to test the resisting power of conical bodies of various forms, in the following manner:— A thin lath was placed horizontally, so as to move freely on a pivot set midway; at one end of the lath a circular card was attached, at the other end a sliding clip traversed, for holding paper cones, having their bases the exact size of the opposite disc. The instrument acted like a steelyard; and when held against the wind, the paper cones were adjusted at different distances from the centre, according to their forms and angles, in order to balance the resistance of the air against the opposing flat surface. The resistance was found to be diminished nearly in the ratio that the height of the cone exceeded the diameter of its base.

It might be expected that the pull of the string of a flying kite should give some indication of the force of inclined surfaces acting against a current of air; but no correct data can be obtained in this way. The incline of the kite is far greater than ever appears in the case of the advancing wing-surface of a bird. The tail is purposely made to give steadiness by a strong pull backwards from the action of the wing, which also exerts considerable force on the suspended cord, which for more than half its length hangs nearly perpendicularly. But the kite, as a means of obtaining unlimited lifting and tractive power, in certain cases where it might be usefully applied, seems to have been somewhat neglected. For its power of raising weights,

the following quotation is taken from Vol. XLI of the *Transactions of the Society of Arts,* relating to Captain Dansey's mode of communicating with a lee-shore. The kite was made of a sheet of holland exactly nine feet square, extended by two spars placed diagonally, and as stretched spread a surface of fifty-five square feet. "The kite, in a strong breeze, extended 1,100 yards of line five-eighths in circumference, and would have extended more had it been at hand. It also extended 360 yards of line, one-and-three-quarters of an inch in circumference, weighing sixty pounds. The holland weighed three-and-a-half pounds; the spars, one of which was armed at the head with iron spikes, for the purpose of mooring it, six-and-three-quarter pounds; and the tail was five times its length, composed of eight pounds of rope and fourteen of elm plank, weighing together twenty-two pounds."

We have here the remarkable fact of ninety-two-and-a-quarter pounds carried by a surface of only 55 square feet.

As all such experiments bear a very close relation to the subject of this paper, it may be suggested that a form of kite should be employed for reconnoitering and exploring purposes, in lieu of balloons held by ropes. These would be torn to pieces in the very breeze that would render a kite most serviceable and safe. In the arrangement there should be a smaller and upper kite, capable of sustaining the weight of the apparatus. The lower kite should be as nearly as practicable in the form of a circular flat plane, distended with ribs, with a car attached beneath like a parachute. Four guy-ropes leading to the car would be required for altering the angle of the plane—vertically with respect to the horizon, and laterally relative to the direction of the wind. By these means the observer could regulate his altitude, so as to command a view of a country, in a radius of at least twenty miles; he could veer to a great extent from side to side, from the wind's course, or lower himself gently, with the choice of a suitable spot for descent. Should the cord break, or the wind fail, the kite would, in either case, act as a parachute, and as such might be purposely detached from the cord, which then being sustained from the upper kite, could be easily recovered. The direction of descent could be commanded by the guy-ropes, these being hauled taut in the required direction for landing.

The author has good reasons for believing that there would be less risk associated with the employment of this apparatus, than the reconnoitering balloons that have now frequently been made use of in warfare.*

The wings of all flying creatures, whether of birds, bats, butterflies, or other insects, have this one peculiarity of structure in common. The front, or leading edge, is rendered rigid by bone, cartilage, or a thickening of the membrane; and in most birds of perfect flight, even the individual feathers are formed upon the same condition. In consequence of this, when the wing is waved in air, it gives a persistent force in one direction, caused by the elastic reaction of the following portion of the edge. The fins and tails of fishes act upon the same principle. In the most rapid swimmers these organs are termed "lobated and pointed." The tail extends out very wide transversely to the body, so that a powerful impulse is obtained against a wide stratum of water, on the condition before explained. This action is imitated in Macintosh's screw-propeller, the blade of which is made of thin steel, so as to be elastic. While the vessel is stationary, the blades are in a line with the keel, but during rotation they bend on one side, more or less, according to the speed and degree of propulsion required, and are thus self-compensating; and could practical difficulties be overcome, would prove to be a form of propeller perfect in theory.

In the flying mechanism of beetles there is a difference of arrangement. When the elytra, or wing-cases, are opened, they are checked by a stop, which sets them at a fixed angle. It is probable that these serve as "aeroplanes," for carrying the weight of the insect, while the delicate membrane that folds beneath acts more as a propelling than a supporting organ. A beetle cannot fly with the elytra removed.

The wing of a bird, or bat, is both a supporting and propelling organ, and flight is performed in a rapid course, as follows:— During the down-stroke it can be easily imagined how the bird is sustained; but in the up-stroke, the weight is also equally well supported, for in raising the wing, it is slightly inclined upwards against the rapidly passing air, and as this angle is somewhat in excess of the motion due to the raising of the wing, the bird is sustained as much during the up as the down-stroke—in fact, though the wing may be rising, the bird is still pressing against the

* The practical application of these suggestions appears to have been anticipated some years previously. In a small work, styled the "History of the Charvolant, or Kite Carriage," published by Longman and Co., appears the following remarks:—"These buoyant sails, possessing immense power, will, as we have before remarked, serve for floating observatories. * * * * Elevated in the air, a single sentinel, with a perspective, could watch and report the advance of the most powerful forces, while yet at a great distance. He could mark their line of march, the composition of their force, and their general strength, long before he could be seen by the enemy." Again, at page 53, we have an account of ascents actually made, as follows:—"Nor was less progress made in the experimental department, when large weights were required to be raised or transposed. While on this subject, we must not omit to observe that the first person who soared aloft in the air by this invention was a lady, whose courage would not be denied this test of its strength. An arm-chair was brought on the ground, then lowering the cordage of the kite by slackening the lower brace, the chair was firmly lashed to the main-line, and the lady took her seat. The main-brace being hauled taut, the huge buoyant sail rose aloft with its fair burden, continuing to ascend to the height of 100 yards. On descending, she expressed herself much pleased with the easy motion of the kite, and the delightful prospect she had enjoyed. Soon after this, another experiment of a similar nature took place, when the inventor's son successfully carried out a design not less safe than bold; that of scaling, by this powerful aërial machine, the brow of a cliff 200 feet in perpendicular height. Here, after safely landing, he again took his seat in a chair expressly prepared for the purpose, and, detaching the swivel-line, which kept it at its elevation, glided gently down the cordage to the hand of the director. The buoyant sail employed on this occasion was thirty feet in height, with a proportionate spread of canvas. The rise of the machine was most majestic, and nothing could surpass the steadiness with which it was manoeuvred; the certainty with which it answered the action of the braces, and the ease with which its power was lessened or increased. * * * Subsequently to this, an experiment of a very bold and novel character was made upon an extensive down, where a wagon with a considerable load was drawn along, whilst this huge machine, at the same time, carried an observer aloft in the air, realising almost the romance of flying."

It may be remarked that the brace-lines here referred to were conveyed down the main-line and managed below; but it is evident that the same lines could be managed with equal facility by the person seated in the car above; and if the main-line were attached to a water-drag instead of a wheeled car, the adventurer could cross rivers, lakes, or bays, with considerable latitude for steering and selecting the point of landing, by hauling on the port or starboard brace-lines as required. And from the uniformity of the resistance offered by the water-drag, this experiment could not be attended with any greater amount of risk than a land flight by the same means.

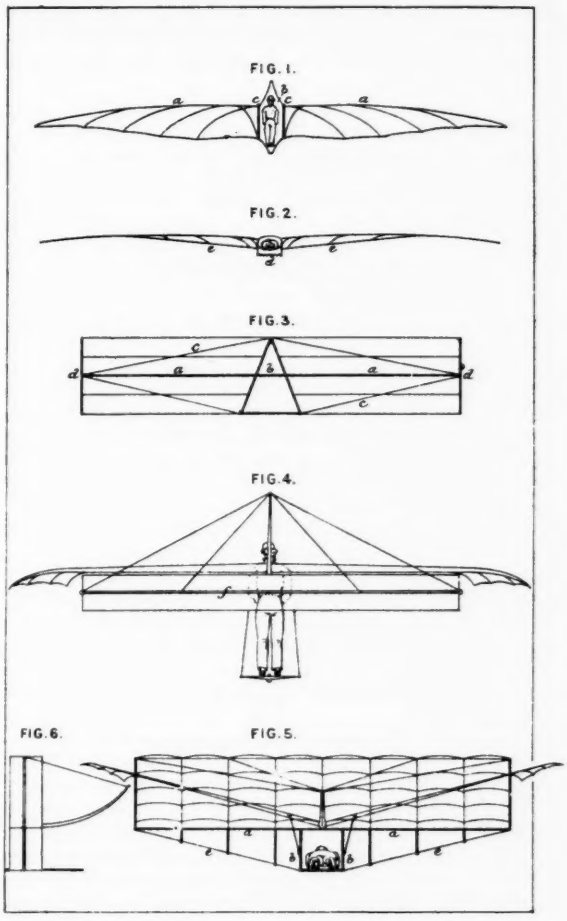
air with a force equal to the weight of its body. The faculty of turning up the wing may be easily seen when a large bird alights; for after gliding down its aerial gradient, on its approach to the ground it turns up the plane of its wing against the air; this checks its descent, and it lands gently.

It has before been shown how utterly inadequate the mere perpendicular impulse of a plane is found to be in supporting a weight, when there is no horizontal motion at the time. There is no material weight of air to be acted upon, and it yields to the slightest force, however great the velocity of impulse may be. On the other hand, suppose that a large bird, in full flight, can make forty miles per hour, or 3,520 feet per minute, and performs one stroke per second. Now, during every fractional portion of that stroke, the wing is acting upon and obtaining an impulse from a fresh and undisturbed body of air; and if the vibration of the wing is limited to an arc of two feet, this by no means represents the small force of action that would be obtained when in a stationary position, for the impulse is secured upon a stratum of fifty-eight feet in length of air at each stroke. So that the conditions of weight of air for obtaining support equally well apply to weight of air, and its reaction in producing forward impulse.

So necessary is the acquirement of this horizontal speed, even in commencing flight, that most heavy birds, when possible, rise against the wind, and even run at the top of their speed to make their wings available, as in the example of the eagle, mentioned at the commencement of this paper. It is stated that the Arabs, on horseback, can approach near enough to spear these birds, when on the plain, before they are able to rise: their habit is to perch on an eminence, where possible.

The tail of a bird is not necessary for flight. A pigeon can fly perfectly with this appendage cut short off: it probably performs an important function in steering, for it is to be remarked, that most birds that have either to pursue or evade pursuit are amply provided with this organ.

The foregoing reasoning is based upon facts, which tend to show that the flight of the largest and heaviest of all birds is really performed with but a small amount of force, and that man is endowed with sufficient muscular power to enable him also to take individual and extended flights, and that success is probably only involved in a question of suitable mechanical adaptations. But if the wings are to be modelled in imitation of natural examples, but very little consideration will serve to demonstrate its utter impracticability when applied in these forms. The annexed diagram, Fig. 1, would be about the proportions needed for a man of medium weight. The wings, *a a*, must extend out sixty feet from end to end, and measure four feet across the broadest part. The man, *b*, should be in a horizontal position, encased in a strong framework, to which the wings are hinged at *c c*. The wings must be stiffened by elastic ribs, extending back from the pinions. These must be trussed by a thin band of steel, *e e*, Fig. 2, for the purpose of diminishing the weight and thickness of the spar. At the front, where the pinions are hinged, there are two levers attached, and drawn together by a spiral spring, *d*, Fig. 2, the tension of which is sufficient to balance the weight of the body and machine, and cause the wings to be easily vibrated by the movement of the feet acting on treadles. This spring serves the purpose of the pectoral muscles in birds. But with all such arrangements the apparatus must fail—*length of wing is indispensable!* and a spar thirty feet long must be strong, heavy, and cumbrous; to propel this alone through the air, at



a high speed, would require more power than any man could command.

In repudiating all imitations of natural wings, it does not follow that the only channel is closed in which flying mechanism may prove successful. Though birds do fly upon definite mechanical principles, and with a moderate exertion of force, yet the wing must necessarily be a vital organ and member of the living body. It must have a marvellous self-acting principle of repair, in case the feathers are broken or torn; it must also fold up in a small compass, and form a covering for the body.

These considerations bear no relation to artificial wings; so in designing a flying-machine, any deviations are admissible, provided the theoretical conditions involved in flight are borne in mind.

Having remarked how thin a stratum of air is displaced beneath the wings of a bird in rapid flight, it follows that in order to obtain the necessary *length* of plane for supporting heavy weights, the surfaces may be superposed, or placed in parallel rows, with an interval between them. A dozen pelicans may fly one above the other without mutual impediment, as if framed together; and it is thus shown how two hundredweight may be supported in a transverse distance of only ten feet.

In order to test this idea, six bands of stiff paper, three feet long and three inches wide, were stretched at a slight upward angle, in a light rectangular frame, with an interval of three inches between them, the arrangement resembling an open Venetian blind. When this was held against a

breeze, the lifting power was very great, and even by running with it in a calm it required much force to keep it down. The success of this model led to the construction of one of a sufficient size to carry the weight of a man. Fig. 3 represents the arrangement. *a a* is a thin plank, tapered at the outer ends, and attached at the base to a triangle, *b*, made of similar plank, for the insertion of the body. The boards, *a a*, were trussed with thin bands of iron, *c c*, and at the ends were vertical rods, *d d*. Between these were stretched five bands of holland, fifteen inches broad and sixteen feet long, the total length of the web being eighty feet. This was taken out after dark into a wet piece of meadow land, one November evening, during a strong breeze, wherein it became quite unmanageable. The wind acting upon the already tighly-stretched webs, their united pull caused the central boards to bend considerably, with a twisting, vibratory motion. During a lull, the head and shoulders were inserted in the triangle, with the chest resting on the base-board. A sudden gust caught up the experimenter, who was carried some distance from the ground, and the affair falling over sideways, broke up the right-hand set of webs.

In all new machines we gain experience by repeated failures, which frequently form the stepping-stones to ultimate success. The rude contrivance just described (which was but the work of a few hours) has taught, first, that the webs, or aeroplanes, must not be distended in a frame, as this must of necessity be strong and heavy, to withstand their combined tension; second, that the planes must be made so as either to furl or fold up, for the sake of portability.

In order to meet these conditions, the following arrangement was afterwards tried:— *a a*, Figs. 4 and 5, is the main spar, sixteen feet long, half an inch thick at the base, and tapered, both in breadth and thickness, to the end; to this spar was fastened the panels *b b*, having a base-board for the support of the body. Under this, and fastened to the end of the main spar, is a thin steel tie-band, *e e*, with struts starting from the spar. This served as the foundation of the superposed aeroplanes, and, though very light, was found to be exceedingly strong; for when the ends of the spar were placed upon supports, the middle bore the weight of the body without any strain or deflection; and further, by a separation at the base-board, the spars could be folded back, with a hinge, to half their length. Above this were arranged the aeroplanes, consisting of six webs of thin holland, fifteen inches broad; these were kept in parallel planes, by vertical divisions, two feet wide, of the same fabric, so that when distended by a current of air, each two feet of web pulled in opposition to its neighbour; and finally, at the ends (which were each sewn over laths), a pull due to only two feet had to be counteracted, instead of the strain arising from the entire length, as in the former experiment. The endpull was sustained by vertical rods, sliding through loops on the transverse ones at the ends of the webs, the whole of which could fall flat on the spar, till raised and distended by a breeze. The top was stretched by a lath, *f*, and the system kept vertical by staycords, taken from a bowsprit carried out in front, shown in Fig. 6. All the front edges of the aeroplanes were stiffened by bands of crinoline steel. This series was for the supporting arrangement, being equivalent to a length of wing of ninety-six feet. Exterior to this, two propellers were to be attached, turning on spindles just above the back. They are kept drawn up by a light spring, and pulled down by cords or chains, running over pulleys in the panels *b b*, and fastened to the end of a swivelling cross-yoke, sliding on the base-board. By working this cross-piece with the feet, motion will be communicated to the propellers, and by giving a longer stroke with one foot than the other, a greater extent of motion will be given to the corresponding propeller, thus enabling the machine to turn, just as oars are worked in a rowing boat. The propellers act on the same principle as the wing of a bird or bat: their ends being made of fabric, stretched by elastic ribs, a simple waving motion up and down will give a strong forward impulse. In order to start, the legs are lowered beneath the base-board, and the experimenter must run against the wind.

An experiment recently made with this apparatus developed a cause of failure. The angle required for producing the requisite supporting power was found to be so small, that the crinoline steel would not keep the front edges in tension. Some of them were borne downwards, and more on one side than the other, by the operation of the wind, and this also produced a strong fluttering motion in the webs, destroying the integrity of their plane surfaces, and fatal to their proper action.

Another arrangement has since been constructed, having laths sewn in both edges of the webs, which are kept permanently distended by cross-stretchers. All these planes are hinged to a vertical central board, so as to fold back when the bottom ties are released, but the system is much heavier than the former one, and no experiments of any consequence have as yet been tried with it.

It may be remarked that although a principle is here defined, yet considerable difficulty is experienced in carrying the theory into practice. When the wind approaches to fifteen or twenty miles per hour, the lifting power of these arrangements is all that is requisite, and, by additional planes, can be increased to any extent; but the capricious nature of the ground-currents is a perpetual source of trouble.

Great weight does not appear to be of much consequence, *if carried in the body;* but the aeroplanes and their attachments seem as if they were required to be very light, otherwise, they are awkward to carry, and impede the movements in running and making a start. In a dead calm, it is almost impracticable to get sufficient horizontal speed, by *mere running* alone, to raise the weight of the body. Once off the ground, the speed must be an increasing one, if continued by suitable propellers. The small amount of experience as yet gained, appears to indicate that if the aeroplanes could be raised in detail, like a superposed series of kites, they would first carry the weight of the machine itself, and next relieve that of the body.

Until the last few months no substantial attempt has been made to construct a flying-machine, in accordance with the principle involved in this paper, which was written seven years ago. The author trusts that he has contributed something towards the elucidation of a new theory, and shown that the flight of a bird in its performance does not require that enormous amount of force usually supposed, and that in fact birds do not exert more power in flying than quadrupeds in running, but considerably less; for the wing movements of a large bird, travelling at a far higher speed in air, are very much slower; and, where weight is concerned, great velocity of action in the locomotive organs is associated with great force.

It is to be hoped that further experiments will confirm the correctness of these observations, and with a sound working theory upon which to base his operations, man may yet command the air with the same facility that birds now do.

TECHNICAL NOTES

Photoelastic Determination of Stress Concentration Factors Caused by a Single U-Notch on One Side of a Plate in Tension

by

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THERE IS very little information on the stress concentration which is caused by a notch on one side of a plate in tension and the authors are not aware of any published data based on experiment. The theoretical solutions due to Neuber[1] are for notches of hyperbolic or elliptic form and only give an approximate result for U-notches of varying depth.

The tests were made on a range of 24 models having different ratios of notch depth to plate width (h/D) and notch depth to notch radius (h/r). Araldite B was cast into plates 12 in. square, approximately 0·10 in. thick, and the models were cut from these plates. As the minimum notch radius desirable for ease of manufacture and photoelastic analysis was 1/16 in., the plate widths had to be varied from 1 in. to 5 in. in order to cover the range of ratios to be investigated. All the models were the full length of the cast plate (12 in.). The notches were machined with a 3/32 in. diameter milling cutter in an engraving machine, which gave no measurable machining stress. Time-edges stress, which would have been present through the absorption of moisture at the edges of the models, was prevented by storing them in an oven maintained at 70°C. A plain tension calibration strip was machined from each plate and used to determine the fringe value of that particular plate.

The models were loaded along an axis mid-way across the minimum section and examined in a polariscope with mercury green monochromatic light. Two methods were used to determine the peak stress at the boundary of the notch. The first, which was used at very high concentrations with steep stress gradients, was to examine a four times magnified real image of the model with a travelling microscope. The positions of fractional fringes were determined at ten different positions along the axis of symmetry, all within 1·0 mm. of the notch boundary. These positions were used to extrapolate to the boundary by fitting a fifth power polynomial to the results by the method of least squares. The second method, which was used on the models having low stress gradients, was to use a photoelectric cell to determine fractional fringe orders at ten different points along the axis of symmetry within 2·0 mm. of the boundary of the notch. In this case the projected image had a magnification of 11. A graph of the values was plotted and extrapolated to the boundary position. In both cases the fringes per inch thickness at the boundary were converted to lb./in.² by multiplying by the fringe value of the plate from which the model was taken. This peak stress was divided by the nominal stress on the minimum section to obtain the stress concentration

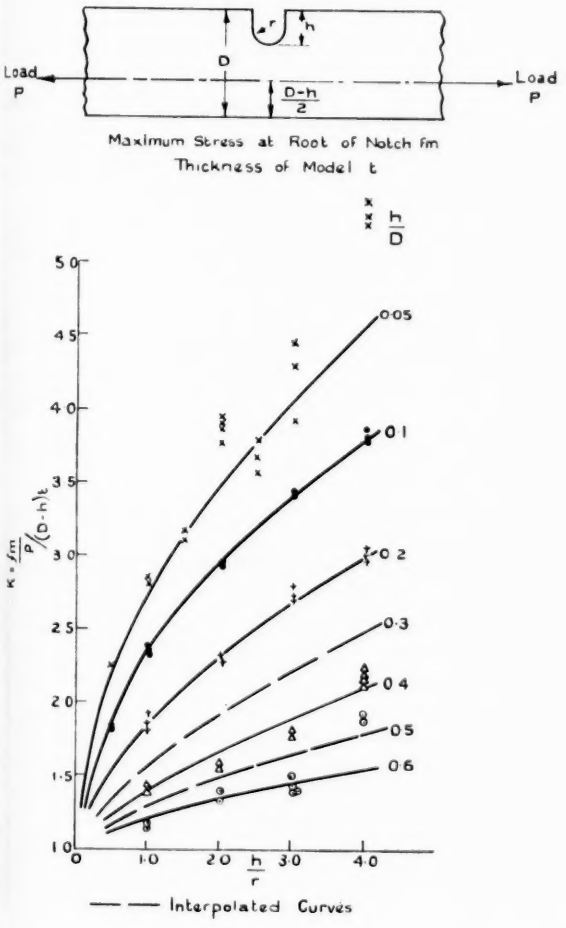

FIGURE 1.

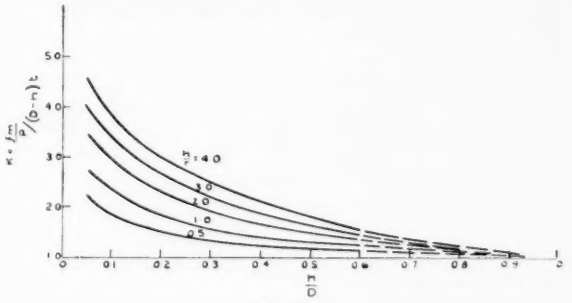

FIGURE 2.

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factor, K. All models were tested at several loads to verify the initial result.

Figure 1 shows the values of K plotted against h/r for various values of h/D. The greater amount of scatter which occurred at the extreme values of h/D was due to two effects. For low values of h/D the models are wide and the stress gradient is extremely steep so that even very small errors in extrapolation are quite significant. The errors are minimised by using the mathematical extrapolation which at least ensures consistency in the handling of the experimental results. On the other hand, for high values of h/D, such a large proportion of the width of the model is cut away that it bends under load and the position of the loading axis relative to the notch changes by a significant amount. Some degree of bending always occurs with a single notch. The high stress at the root would cause the loading line to move away from the notch were it not for the predominating effect of the unstressed material at the edge of the plate on either side of the notch which causes the loading line to move towards the notch as the load is increased. Measurements of the amount of movement made it possible to apply an approximate correction for this effect. It is perhaps worth noting that the use of ball bearings at the points of load application made a marked improvement in consistency with the narrow models.

Figure 2 was obtained by cross-plotting the stress concentration values of Fig. 1 against h/D for various ratios of h/r. These curves must be smooth and also form a consistent family and this fact was used as an aid in positioning the curves for high and low values of h/D in Fig. 1.

When allowance is made for the various sources of error it is estimated that the values of K are accurate to within ± 5 per cent.

ACKNOWLEDGMENT

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A Note on the Temperature Transients in a Supersonic "Blow-Down" Wind Tunnel

by

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IN recent years, due to the high cost of continuous supersonic wind tunnels, much attention has been focused on the simpler intermittent "blow-down" tunnel.

Considerable data has been published on most aspects of the design of this type of equipment and quite a number of large installations have recently been brought into operation in the United States, with even larger numbers currently building both in this country and in the United States.

In a conventional "blow-down" tunnel layout, the air leaves the storage vessels, passes through a reducing valve and enters a settling chamber.

One of the fundamental features in a "blow-down" system is the temperature drop which occurs in the storage

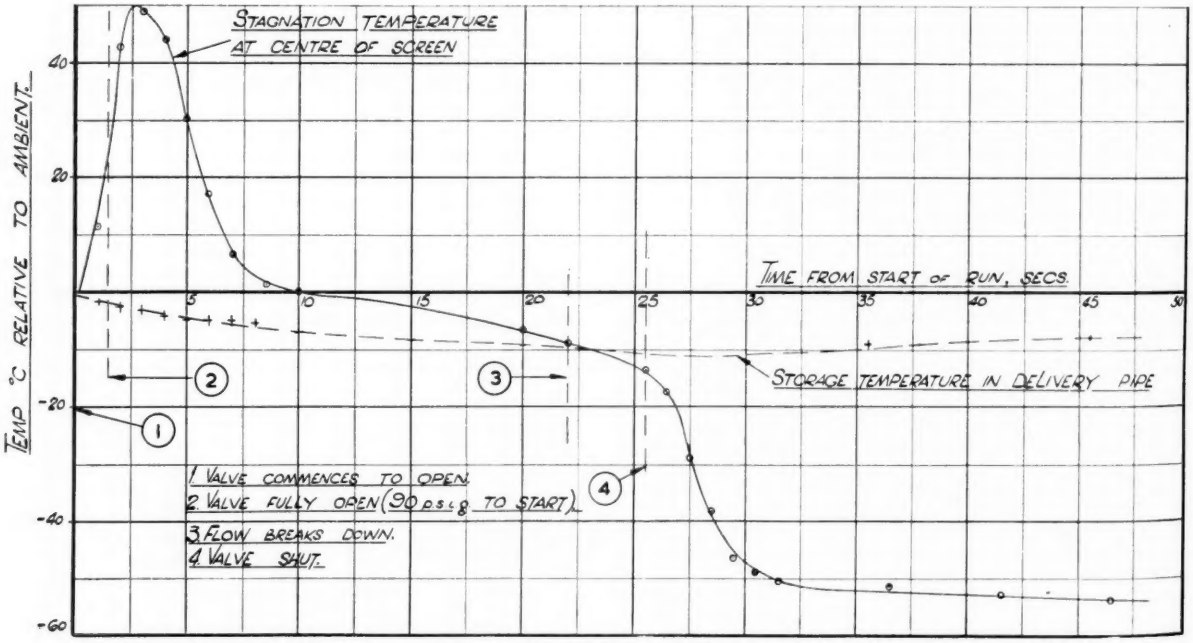

FIGURE 1. Storage and stagnation temperature in Avro tunnel $M = 3{\cdot}55$, storage = 100 P.S.I.G., stagnation = 65 P.S.I.G.

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vessel due to adiabatic expansion. This would result in a fall in stagnation temperature, with a consequent variation in Reynolds number during a tunnel run. Difficulties may also be experienced with instrumentation in the model if subjected to temperature changes. This temperature drop is normally minimised by the provision of a heat exchanger matrix, either in the storage vessel itself, or in the settling chamber, or both. A number of theoretical papers and the results of some experimental work have been published, for example, Refs. 1, 2 and 3.

Temperature measurements have recently been made in the Avro Supersonic Wind Tunnel, which employs a simple capacity heat exchanger in the storage vessel. At modest values of Mach number, say up to $M=2{\cdot}5$, and at minimum pressure ratios, the temperature difference between the stored air and the stagnation-chamber air is not great, of the order of 10°C. However, as the Mach number is raised further, due to initial compression of air in the settling chamber, there is a sharp temperature rise at the beginning of each tunnel run. Conversely, at the termination of each run, expansion of the air remaining in the settling chamber takes place and this is followed by a corresponding drop in temperature.

A typical record of the temperature measured in the settling chamber of the Avro tunnel at a Mach number of 3·55 with a pressure ratio of 5·5 is shown in Fig. 1. This indicates the wide difference between storage and stagnation temperature under the terminal conditions. The Joule-Thompson effects at the reducing valve are small enough to be neglected in this instance.

The magnitude and duration of these temperature transients is clearly influenced to a large extent by the particular tunnel geometry and will be dependent upon such factors as the size of the settling chamber in relation to the tunnel as a whole, the mass of steel in the region of the settling chamber and, in particular, on the Mach number and pressure ratio employed.

Unpublished data from another source in what may be taken to be a typical installation at $M=4{\cdot}0$ shows an initial temperature rise of some 180°C. It is possible to envisage the conditions in a tunnel with a relatively short running time at high Mach numbers in which the temperature transients occupy a considerable proportion of the available running time, thus vitiating the constant Reynolds number requirement.

The fall in temperature which occurs at the termination of a run obviously has less practical significance, but nevertheless there are certain points which require closer examination during the tunnel design. Firstly, the whole of the tunnel structure downstream of the reducing valve is subject to severe thermal shocks and this may have an unfortunate effect on schlieren windows; secondly, strain-gauge balances and transducers in the tunnel should have far better compensation for thermal effects than might originally have been anticipated. It would clearly be unwise to assume constant Reynolds number conditions for the first few seconds of a typical run and, additionally, it would be unwise to take after-run balance zeros for some considerable time, probably up to 10/15 minutes unless temperature compensation had been fully achieved.

It is thought that this aspect of the operation of "blow-down" wind tunnels has not received attention in the past and it is clearly possible to minimise these effects by suitable provision in the initial design.

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A Note on the Calculation of Torsional Natural Frequencies of Branch Systems

by

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AN ADOPTION of matrix techniques is proposed as a method for the solution of torsional natural frequencies in a branch or geared shaft system. Both fundamental and higher mode shapes and frequencies are produced by a comparatively rapid and straightforward technique. The method is illustrated by a numerical example of a coupled turbine and reciprocating engine system driving a common propeller.

1. Introduction

With the advent of coupled engine installations, both of turbine and reciprocating engines, more and more emphasis is being placed on usable methods of calculating natural frequencies. Marine engineers have long faced problems very similar to those confronting aeronautical engineers today. Coupled turbine, or turbine and reciprocating steam engines driving through long flexible shafts, are dynamically identical to engine installations buried in wings of modern aircraft. Methods used at present for obtaining free torsional natural frequencies of branch or geared drive systems have been of "equivalent system" type, *i.e.* a reduction of one or more branches into one equivalent inertia. For the more complex systems, the methods of Holzer, Stodola and others use the technique of repeated approximations. To simplify the solution of torsional vibration problems a method is presented based on the well-known matrix technique which reduces the repeated approximations to a straightforward computation and which does not require unusual simplifying assumptions. A simple conversion of basic data to account for gearing is all that is required before applying the matrix approach.

In brief, the solution consists of writing the potential and kinetic energies of the system, employing Lagrange's equation to write the equations of motion. These equations of motion are then, along with the equation of the moment of momentum, written in matrix notation for iteration for frequency (which will appear as a dominant root of the

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dynamic matrix) and the modal column which is the normal mode of deflection.

A typical example is included to illustrate a numerical method which simplifies the solution. This numerical procedure is shown to be similar to the rigorous theoretical derivation but greatly reduces the calculating time to obtain modal shapes and frequencies.

2. Theory of solution

It will be assumed that the cranks, plus the reciprocating parts of the turbine buckets, as the case may be, are by standard methods replaced by equivalent discs of inertia and equivalent plain shafts. These typical assumptions are common to all analysis methods where varying moments of inertia are not taken into account. The existence of intermediate gearing is accounted for by connecting the inertias and stiffnesses of each branch to a common shaft speed. This connection is simply the sequence of the ratio of the branch shaft speed to the reference shaft speed.

The equation of kinetic energy is written as:

$$T=\tfrac{1}{2}\sum_{i=0}^{n} I_i\dot{\phi}_i^2 \quad . \quad . \quad . \quad (1)$$

where I is the moment of inertia of each particular station, denoted by i, and ϕ is the corresponding angular deflection.

The potential energy of each section of shaft is equal to

$$\Delta u=\tfrac{1}{2}k_i(\Delta\phi_i)^2 \quad . \quad . \quad . \quad (2)$$

where $\Delta\phi_i$ is the relative angular displacement between stations (i-1) and i, and k_i the torsional stiffness per unit length of shaft between these stations. Thus the total potential energy may be expressed as

$$U=\tfrac{1}{2}\sum_{i=1}^{n} k_i(\Delta\phi_i)^2 \quad . \quad . \quad . \quad (3)$$

Employing Lagrange's equation, as

$$\frac{d}{dt}\left(\frac{\partial l}{\partial\dot{\phi}_i}\right)-\frac{\partial T}{\partial\phi_i}+\frac{\partial U}{\partial\phi_i}=0 \quad . \quad . \quad (4)$$

which is well known and obviates any derivations, the equations of motion are written. A system of equations are obtained of the form

$$I_i\ddot{\phi}_i+\sum_{j=0}^{n} k_{ij}\phi_j=0 \quad . \quad . \quad . \quad (5)$$

For harmonic motion,

$$\phi_i=\phi_i e^{i\omega t} \quad . \quad . \quad . \quad . \quad (6)$$

The equations of motion may be rewritten as a system of equation of the form

$$\omega^2 I_i\phi_i+\sum_{i=0}^{n} k_{ij}\phi_j=0 \quad . \quad . \quad . \quad (7)$$

Since the system dealt with does not have any external forces acting on it, the total moment of momentum is constant. There will be no loss in generality in the vibration problem if this constant is set equal to zero, or

$$\omega\sum_{i=0}^{n} I_i\phi_i=0 \quad . \quad . \quad . \quad (8)$$

The trivial root of zero frequency may be removed by eliminating one co-ordinate with the aid of equation (8) and thus we have a system of n degrees of freedom.

Writing the system of n equations in matrix form we have:

$$\omega^2[I'][\phi]=[k'][\phi] \quad . \quad . \quad . \quad (9)$$

To facilitate iteration, equation (9) may be rewritten as:

$$\frac{1}{\omega^2}[\phi]=[k]^{-1}[I'][\phi] \quad . \quad . \quad . \quad . \quad (10)$$

where:
- $[\phi]$ is a column matrix of deflection
- $[I']$ is a matrix of inertias
- $[k']$ is the stiffness matrix after elimination of the co-ordinates by use of equation (9)
- $[k']^{-1}$ is the inverted stiffness matrix (may be called the influence coefficient matrix).

Performing the indicated multiplication of equation (10) we may iterate to obtain the normal mode and frequency of the system. The dynamic matrix

$$[k']^{-1}[I'] \quad . \quad . \quad . \quad (11)$$

may be operated upon to produce higher modes.[4, 5]

3. Method of solution

The method of solution lies mainly in obtaining a dynamic matrix or, specifically, a stiffness matrix consisting of torsional influence coefficients.

The influence coefficient θ_{ij} is defined as the angular displacement of disc i, referred to disc 0 as fixed, when a unit positive torque is applied to disc j. The assumption of perfect elasticity makes valid Maxwell's law of reciprocal deflections which, interpreted here, simply means that $\theta_{ij}=\theta_{ji}$. The influence coefficient may be derived directly from the expression giving the increment ϕ in angle for a one inch length of shaft of circular cross section, which is

$$\phi=T/k_t \quad . \quad . \quad . \quad (12)$$

where T is the torque at point j, and k_t, the torsional rigidity, defined as the polar moment of inertia of the shaft cross section (I_p) multiplied by the shear modulus (G) and divided by the shaft increment, l. Therefore, for two discs i and j fixed on a shaft, a distance l apart, the influence coefficient for a unit torque applied at j is

$$\theta_{ij}=\frac{l}{I_pG}=\frac{1}{k_t} \quad . \quad . \quad . \quad (13)$$

Numerical calculation of torsional influence coefficients for branch systems will be discussed later.

If a torque is applied to disc j, the resulting twist at disc i will be $T_j\theta_{ij}$. In general, if torques T are applied to each disc, the displacement of disc i relative to disc 0 will be:

$$\phi_i-\phi_0=T_1\theta_{i1}+T_2\theta_{i2}+T_3\theta_{i3}+\ldots+T_n\theta_{in} \quad (14)$$

or for the system, a series of equations of identical form. During simple harmonic motion the torque T_n applied to each disc in equation (14) may be replaced by the inertia torques of the various discs:

$$T_j=-I_j\frac{d^2\phi_j}{dt^2}=I_j\omega^2\phi_j \quad . \quad . \quad (15)$$

Equation (14) may then be rewritten in terms of the inertias and the influence coefficients:

$$\begin{aligned}\phi_1-\phi_0&=\omega^2(I_1\theta_{11}\phi_1+I_2\theta_{12}\phi_2+\ldots+I_n\theta_{1n}\phi_n)\\ \phi_n-\phi_0&=\omega^2(I_1\theta_{n1}\phi_1+I_2\theta_{n2}\phi_2+\ldots+I_n\theta_{nn}\phi_n)\end{aligned} \quad (16)$$

The deflection of disc 0 may be expressed in terms of the other discs by utilising the conservation of angular momentum, as

$$\sum_{i=1}^{n} I_i\phi_i=-I_0\phi_0 \quad . \quad . \quad . \quad (17)$$

with some algebraic manipulation. More explicitly, equation (16) is multiplied by I_i; *i.e.* ϕ_1 is multiplied by I_1, ϕ_2 by I_2, and so on. The system of equations is then summed up and substituted into equation (17). I_0 is known, thus ϕ_0 is easily evaluated and removed from the

system of equations by simple subtraction. The resulting system of equations is of the form:

$$\begin{aligned}\phi_1 &= (a_{11}\phi_1 + a_{12}\phi_2 + a_{13}\phi_3 + \ldots + a_{1n}\phi_n)\omega^2 \\ \phi_2 &= (a_{21}\phi_1 + a_{22}\phi_2 + a_{23}\phi_3 + \ldots + a_{2n}\phi_n)\omega^2 \\ &\cdots\cdots\cdots\cdots\cdots\cdots\cdots\cdots\cdots\cdots \\ \phi_n &= (a_{n1}\phi_1 + a_{n2}\phi_2 + a_{n3}\phi_3 + \ldots + a_{nn}\phi_n)\omega^2\end{aligned} \quad (18)$$

These may be expressed more simply in matrix notation as:

$$[\phi] = [a_{ij}][\phi]\omega^2 \quad . \quad . \quad . \quad (19)$$

Where a_{ij} is a function of inertia and stiffness obtained from the inclusion of equation (17) in equation (16).

4. Numerical solution

A coupled turbine-reciprocating engine system was chosen to illustrate the method under discussion. This system is illustrated in Fig. 1, the data for which is published in Ref. 2. The entire system is referred to propeller shaft speed with corrections made in the inertias and shaft stiffnesses as the square of the ratio of shaft speed to the propeller shaft speed, such that:

The polar moment of inertia

$$\left.\begin{array}{ll} I_0 = 810 \text{ ft. lb. sec.}^2 & I_4 = 105 \text{ ft. lb. sec.}^2 \\ I_1 = 105 \text{ ft. lb. sec.}^2 & I_5 = 630 \text{ ft. lb. sec.}^2 \\ I_2 = 180 \text{ ft. lb. sec.}^2 & I_6 = 11370 \text{ ft. lb. sec.}^2 \\ I_3 = 105 \text{ ft. lb. sec.}^2 & \end{array}\right\} \quad . \quad (20)$$

and shaft stiffness:

$$\left.\begin{array}{ll} k_1 = 0{\cdot}445 \times 10^6 \text{ ft. lb./rad.} & k_4 = 30{\cdot}8 \times 10^6 \text{ ft. lb./rad.} \\ k_2 = 0{\cdot}780 \times 10^6 \text{ ft. lb./rad.} & k_5 = 3{\cdot}75 \times 10^6 \text{ ft. lb./rad.} \\ k_3 = 15{\cdot}8 \times 10^6 \text{ ft. lb./rad.} & k_6 = 300 \times 10^6 \text{ ft. lb./rad.} \end{array}\right\} \quad (21)$$

To calculate the torsional influence coefficient, the propeller was arbitrarily considered fixed and a unit torque successfully applied to each of the discs. Application of a unit torque at the primary gear (I_1), obviously produces a deflection of

$$\theta_1 = \theta_2 = \theta_3 = \theta_4 = \ldots = \phi_1 = \frac{1}{k_1} \quad . \quad . \quad (22)$$

while an application of a unit torque on I_2 produces complex deflections; *i.e.* deflection in the branches are different. In the engine branch the deflections are:

$$\theta_2 = \theta_3 = \theta_4 = \frac{1}{k_1} + \frac{1}{k_2} \quad . \quad . \quad (23)$$

In the primary gear: $$\theta_1 = \frac{1}{k_1} \quad . \quad . \quad . \quad (24)$$

In the turbine branch: $$\theta_5 = \theta_6 = \frac{1}{k_1} \quad . \quad . \quad . \quad (25)$$

Applying a unit torque to the succeeding discs we may write the stiffness matrix as:

$$[\theta] = \begin{bmatrix} \frac{1}{k_1} & \frac{1}{k_1} & \frac{1}{k_1} & \frac{1}{k_1} & \frac{1}{k_1} & \frac{1}{k_1} \\ \frac{1}{k_1}\left(\frac{1}{k_1}+\frac{1}{k_2}\right) & \left(\frac{1}{k_1}+\frac{1}{k_2}\right) & \left(\frac{1}{k_1}+\frac{1}{k_2}\right) & \frac{1}{k_1} & \frac{1}{k_1} \\ \frac{1}{k_1}\left(\frac{1}{k_1}+\frac{1}{k_2}\right) & \left(\frac{1}{k_1}+\frac{1}{k_2}+\frac{1}{k_3}\right) & \left(\frac{1}{k_1}+\frac{1}{k_2}+\frac{1}{k_3}\right) & \frac{1}{k_1} & \frac{1}{k_1} \\ \frac{1}{k_1}\left(\frac{1}{k_1}+\frac{1}{k_2}\right) & \left(\frac{1}{k_1}+\frac{1}{k_2}+\frac{1}{k_3}\right) & \left(\frac{1}{k_1}+\frac{1}{k_2}+\frac{1}{k_3}+\frac{1}{k_4}\right) & \frac{1}{k_1} & \frac{1}{k_1} \\ \frac{1}{k_1} & \frac{1}{k_1} & \frac{1}{k_1} & \left(\frac{1}{k_1}+\frac{1}{k_5}\right) & \left(\frac{1}{k_1}+\frac{1}{k_5}\right) \\ \frac{1}{k_1} & \frac{1}{k_1} & \frac{1}{k_1} & \left(\frac{1}{k_1}+\frac{1}{k_5}\right) & \left(\frac{1}{k_1}+\frac{1}{k_5}+\frac{1}{k_6}\right) \end{bmatrix} \quad . \quad . \quad . \quad . \quad . \quad . \quad (26)$$

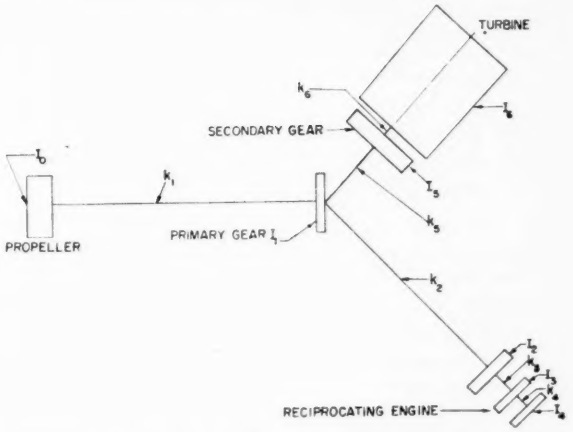

FIGURE 1. Equivalent system referred to propeller shaft speed.

Substituting the correct values into equation (6), the stiffness matrix is:

$$[\theta] = \begin{bmatrix} 2{\cdot}25 & 2{\cdot}25 & 2{\cdot}25 & 2{\cdot}25 & 2{\cdot}25 & 2{\cdot}25 \\ 2{\cdot}25 & 3{\cdot}53 & 3{\cdot}53 & 3{\cdot}53 & 2{\cdot}25 & 2{\cdot}25 \\ 2{\cdot}25 & 3{\cdot}53 & 3{\cdot}593 & 3{\cdot}593 & 2{\cdot}25 & 2{\cdot}25 \\ 2{\cdot}25 & 3{\cdot}53 & 3{\cdot}593 & 3{\cdot}625 & 2{\cdot}25 & 2{\cdot}25 \\ 2{\cdot}25 & 2{\cdot}25 & 2{\cdot}25 & 2{\cdot}25 & 2{\cdot}516 & 2{\cdot}516 \\ 2{\cdot}25 & 2{\cdot}25 & 2{\cdot}25 & 2{\cdot}25 & 2{\cdot}516 & 2{\cdot}519 \end{bmatrix} \times 10^6 \quad . \quad . \quad . \quad . \quad . \quad . \quad (27)$$

Expressing the information of equation (20) in matrix form, the inertia matrix may be written as:

$$[I] = \begin{bmatrix} I_1 & & & & & \\ & I_2 & & & & \\ & & I_3 & & & \\ & & & I_4 & & \\ & & & & I_5 & \\ & & & & & I_6 \end{bmatrix} = \begin{bmatrix} 105 & & & & & \\ & 180 & & & & \\ & & 105 & & & \\ & & & 105 & & \\ & & & & 630 & \\ & & & & & 11370 \end{bmatrix} \quad . \quad . \quad . \quad . \quad . \quad . \quad (28)$$

The dynamic matrix, D, is then the result of matrix multiplication of the influence (stiffness) matrix, equation (27), by the inertia matrix, equation (28), or:

$$[D]=[\theta]\,[I]=$$

$$\begin{bmatrix} 236{\cdot}25 & 405 & 236{\cdot}25 & 236{\cdot}25 & 1417{\cdot}5 & 25582{\cdot}5 \\ 236{\cdot}25 & 635{\cdot}4 & 370{\cdot}65 & 370{\cdot}65 & 1417{\cdot}5 & 25582{\cdot}5 \\ 236{\cdot}25 & 635{\cdot}4 & 377{\cdot}27 & 377{\cdot}27 & 1417{\cdot}5 & 25582{\cdot}5 \\ 236{\cdot}25 & 635{\cdot}4 & 377{\cdot}27 & 380\ 63 & 1417{\cdot}5 & 25582{\cdot}5 \\ 236{\cdot}25 & 405 & 236{\cdot}25 & 236{\cdot}25 & 1585{\cdot}08 & 28606{\cdot}92 \\ 236{\cdot}25 & 405 & 236{\cdot}25 & 236{\cdot}25 & 1585{\cdot}08 & 28641{\cdot}03 \end{bmatrix} \times 10^6 \quad (29)$$

The system of equations, *i.e.* the system of n equations as indicated by (16), may be expressed in matrix notation

$$[\phi_i-\phi_0]=[D]\,[\phi]\,\omega^2 \quad (30)$$

In order to solve equations (30) an additional relationship must be obtained between the unknowns. No external forces are permitted as we are considering free torsional vibrations. Disc 0 is released and allowed to rotate as freely as the rest of the system. Equilibrium of the system when all the discs are free to oscillate can be expressed by imposing the condition that angular momentum be zero, or

$$\omega\,(I_0\phi_0+I_1\phi_1+I_2\phi_2+\ldots\ldots+I_6\phi_6)=0$$

thus, since, $\omega\neq 0$

$$I_0\phi_0=-\sum_{i=1}^{6} I_i\phi_i=810\,\phi_0 \quad (31)$$

Using equation (31) in the manner previously outlined we may eliminate. It may be clearer, however, to discuss the required manipulations using equation (16). Multiply the first, by I_1, the second by I_2, and so on. The resultant equations being

$$\begin{aligned} I_1\phi_1-I_1\phi_0&=\omega^2\,(I_1{}^2\theta_{11}\phi_1+I_1I_2\theta_{12}\phi_2+I_1I_3\theta_{13}\phi_3+\ldots+I_1I_6\theta_{16}\phi_6) \\ &\vdots \\ I_6\phi_6-I_6\phi_0&=\omega^2\,(I_1I_6\theta_{61}\phi_0+I_2I_6\theta_{62}\phi_2+I_3I_6\theta_{63}\phi_3+\ldots+I_6{}^2\theta_{66}\phi_6) \end{aligned} \quad (32)$$

adding equations (32). the result is

$$\sum_{i=1}^{6} I_i\phi_i-\phi_0\sum_{i=1}^{6} I_i=\omega^2\,(A_1\phi_1+A_2\phi_2+A_3\phi_3+\ldots\ldots+A_6\phi_6) \quad (33)$$

when the A's are coefficients of ϕ resulting from the manipulation

$$A_1=I_1{}^2\theta_{11}+I_1I_2\theta_{12}+I_1I_3\theta_{13}+\ldots\ldots+I_1I_6\theta_{16} \text{ and so on.}$$

Consider now the left side of equation (33). We may add and subtract the quantity $I_0\phi_0$ such that

$$\sum_{i=1}^{6} I_i\phi_i+I_0\phi_0-I_0\phi_0-\phi_0\sum_{i=1}^{6} I_i=\sum_{i=0}^{6} I_i\phi_i-\phi_0\sum_{i=0}^{6} I_i$$

but the quantity $\sum_{i=0}^{6} I_i\phi_i=0$, ϕ_0 may be obtained in terms of ϕ_1, ϕ_2, ϕ_3 and so on, as;

$$\phi_0=[-221{\cdot}9\phi_1-387{\cdot}1\phi_2-225{\cdot}9\phi_3-225{\cdot}7\phi_4-1482{\cdot}4\phi_5-26782\phi_6]\omega^2 \quad (34)$$

This value of ϕ_0 is then subtracted from each row of equation (30) thus obtaining a series of equations which define the free-free motion of the system illustrated,

$$[\phi]=$$

$$\begin{bmatrix} 14{\cdot}39 & 17{\cdot}9 & 10{\cdot}34 & 10{\cdot}31 & -64{\cdot}85 & -1199{\cdot}48 \\ 14{\cdot}39 & 247{\cdot}9 & 144{\cdot}74 & 144{\cdot}71 & -64{\cdot}85 & -1199{\cdot}48 \\ 14{\cdot}39 & 247{\cdot}9 & 151{\cdot}36 & 151{\cdot}33 & -64{\cdot}85 & -1199{\cdot}48 \\ 14{\cdot}39 & 247{\cdot}9 & 151{\cdot}36 & 154{\cdot}69 & -64{\cdot}85 & -1199{\cdot}48 \\ 14{\cdot}39 & 17{\cdot}9 & 10{\cdot}34 & 10{\cdot}31 & 102{\cdot}73 & 1859{\cdot}05 \\ 14{\cdot}39 & 17{\cdot}9 & 10{\cdot}34 & 10{\cdot}31 & 102{\cdot}73 & 1893{\cdot}16 \end{bmatrix} [\phi]\,\omega^2 \quad (35)$$

We now iterate upon the dynamic matrix until convergence is obtained. The final values appear as a column of deflections, this case being the first harmonic,

$$\begin{bmatrix} \phi_1 \\ \phi_2 \\ \phi_3 \\ \phi_4 \\ \phi_5 \\ \phi_6 \end{bmatrix} = \begin{bmatrix} -0{\cdot}670 \\ -0{\cdot}901 \\ -0{\cdot}908 \\ -0{\cdot}909 \\ -0{\cdot}982 \\ 1{\cdot}000 \end{bmatrix} \times \frac{10^6}{\omega^2} \times 1949{\cdot}57$$

with the frequency given by the normalising value

$$\omega^2=10^6/1949{\cdot}57$$

$$\omega=\sqrt{512}\times\frac{60}{2\pi}=216{\cdot}5 \text{ r.p.m.}$$

ϕ_0 is now found by equation (17):

$$810\,\phi_0=-[105\,(-0{\cdot}670)+180\,(-0{\cdot}901)+105\,(-0{\cdot}908)+105\,(-0{\cdot}909)+630\,(0{\cdot}982)+11370(1)]$$

$$\phi_0=-14{\cdot}278.$$

Proof of the convergence of successive approximations to the modal column and of the normalising value, the dominant root, to the frequency may be found in any standard text on the topic of matrix algebra[1]. As mentioned in Section 3 higher modes may be obtained by operating upon the dynamic matrix as described in Refs. 4 and 5.

5. Conclusion

The first three modes of the system illustrated in Fig. 1 were calculated by the numerical method and that of Ref. 5. The results are shown in Table I.

TABLE I

Mode	1	2	3
Holzer Method	221	395	2080
Matrix Method	216·5	397	2075
Per cent Difference	2·03	0·504	0·24

Agreement can be seen to be quite good. The author feels the slight difference is due to the degree of accuracy used in both methods.

The matrix type solution permits a high degree of accuracy, although relatively few places are carried. The matrix method becomes very desirable when analysing a system for modes higher than the fundamental as it precludes the drawing of a remainder curve to determine the trend of approximations. At the higher frequencies the remainder curve becomes very sensitive to values of frequency, a condition which makes accurate evaluation at zero remainder a time consuming "pin-pointing" procedure.

Comparative values of deflections produced by the two methods also show good agreement. When comparing deflections it must be realised that the matrix solution shows deflections relative to the maximum deflection of the inertias included in the system represented by the dynamic matrix while the Holzer deflections are relative to one of the extreme inertias, *i.e.* extremes of one of the branches. The deflections are similar, the difference being a constant ratio of the two maxima.

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Note on the Solution of Non-Linear Simultaneous Equations by Successive Approximations

by

W. J. GOODEY, A.F.R.Ae.S.

THE CALCULATION of the roots of an algebraic or transcendental equation in a single unknown is a problem of frequent occurrence. For a real root the usual procedure is to obtain a first approximation to the required quantity, graphically or otherwise, and to improve this approximation by successive applications of the Newton-Raphson process[1]. The extension of this process to the improvement of an approximate solution of a set of non-linear simultaneous equations in n unknowns is fairly obvious, but it does not seem to have received much attention in text books, although the case of two unknowns is dealt with in Ref. 2. When the number of unknowns is greater than two the problem of obtaining the first approximation is of course difficult, but when the equations represent the behaviour of some physical system it may be possible to estimate this by making appropriate simplifying assumptions. We shall consider only *real* solutions. A suggested method of dealing with complex unknowns is, however, given in Section 5 of this note.

1. Let

$$a_i(x_j) = 0 \quad (i, j = 1, 2, 3 \ldots n) \quad . \quad . \quad (1)$$

be a set of equations non-linear in some or all of the quantities x_j, and let $x_j^{(1)}$ be a set of approximate roots.

Write

$$x_j - x_j^{(1)} = \varepsilon v_j^{(1)}, \quad . \quad . \quad . \quad (2)$$

where ε is a quantity indicative of the average error of the approximate solution. For example, ε might be taken as the root mean square of the proportional errors $(1 - x_j^{(1)}/x_j)$. Substitution of (2) in (1) gives

$$a_i(x_j) = a_i(x_j^{(1)}) + \varepsilon a_{ij}^{(1)} v_j^{(1)} + 0(\varepsilon^2) = 0, \quad . \quad (3)$$

where $a_{ij}^{(1)} = \partial a_i / \partial x_j (x_j = x_j^{(1)})$, and adopting a familiar convention the double suffix j in the ε term implies summation over all the values taken by this suffix. Neglecting terms of order ε^2, equations (3) are linear in the $\varepsilon v_j^{(1)}$ terms, and the resulting solution will be denoted by $\delta x_j^{(1)}$,

i.e.

$$a_{ij}^{(1)} \delta x_j^{(1)} = -a_i(x_j^{(1)}) \quad . \quad . \quad (4)$$

Also, let

$$x_j^{(1)} + \delta x_j^{(1)} = x_j^{(2)} \quad . \quad . \quad . \quad (5)$$

Then $x_j^{(2)}$ will differ from the exact solution of (1) by quantities of order ε^2.

2. Let the square matrix $[a_{ij}^{(1)}]$ $(i, j = 1, 2, 3 \ldots n)$ be denoted by A_1, the column $a_i(x_j^{(1)})$ by $a^{(1)}$, and the columns $x_j^{(1)}$ and $x_j^{(2)}$ by $x^{(1)}$ and $x^{(2)}$. Then from (4) and (5), assuming A_1 to be non-singular, we have

$$x^{(2)} - x^{(1)} = -A_1^{-1} a^{(1)} \quad . \quad . \quad . \quad (6)$$

A sequence of approximations $x^{(3)}$, $x^{(4)}$, and so on, may be obtained by repeating this process, where for example

$$x^{(3)} - x^{(2)} = -A_2^{-1} a^{(2)},$$

A_2 being the matrix $[a_{ij}^{(2)}]$ and $a^{(2)}$ the column $a_i(x_j^{(2)})$. The approximation $x^{(3)}$ will evidently have errors of order ε^4, while $x^{(4)}$ will have errors of order ε^8, and so on. If the quantity ε is small, the rate of convergence of successive approximations will be very rapid, but the method has the disadvantage that each step in the process involves the reciprocation of an n^2 matrix, and in general the labour of computation will increase very rapidly with n. This drawback may be overcome, at the expense of a slower rate of convergence, as follows.

3. Consider the sequence of approximations

$$x^{(k+1)} = x^{(k)} - A_1^{-1} a^{(k)}, \quad . \quad . \quad (7)$$

$a^{(k)}$ being the column $a_i(x_j^{(k)})$.

Thus, if the error $x - x^{(k)}$ in the k^{th} approximation is denoted by $\Delta x^{(k)}$, then

$$\Delta x^{(k+1)} = \Delta x^{(k)} + A_1^{-1} a^{(k)}. \quad . \quad . \quad (8)$$

Now

$$a^{(k)} = a_i(x_j^{(k)}) = a_i(x_j - \Delta x_j^{(k)})$$
$$= -A \Delta x^{(k)} + \text{terms of the order } (\Delta x_j^{(k)})^2,$$

where A is the matrix $[a_{ij}]$ in which the derivatives a_{ij} have their exact values, and $\Delta x_j^{(k)}$ denotes the individual items of the column $\Delta x^{(k)}$.

Thus, from equation (8),

$\Delta x^{(k+1)} = \Delta x^{(k)} - A_1^{-1} A \Delta x^{(k)} +$ terms of the order $(\Delta x_j^{(k)})^2$. The coefficients of the matrix A differ from the corresponding coefficients of A_1 by quantities of order ε, and so $A_1^{-1} A$ will be of the form $I + \varepsilon B$, I being the n^2 unit matrix. Hence, $\Delta x^{(k+1)}$ is of order $\varepsilon \Delta x^{(k)}$. The successive approximations derived from equation (7) will therefore have errors of order ε^2, ε^3, and so on. The matrix A_1 may of course be replaced by any of the matrices A_r, say, at any stage, if the rate of convergence with A_1 is not very satisfactory.

The question of repeated sets of roots, when the matrix A will be singular, will not be discussed at length here. As an example of this, however, we may take the case where the given equations are symmetrical functions of all the unknowns. It is then possible to derive from these a single equation of the n^{th} order whose roots are

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$x_1, x_2, \ldots x_n$, taken in any order. If the latter equation has a pair of equal roots, the n equations will have $\frac{1}{2}n!$ pairs of repeated solutions.

4. Numerical example

$$x_1(x_1+1)-x_2-1=0,$$
$$x_2(x_2+1)-x_3-4=0,$$
$$x_3(x_3+1)-x_4-9=0,$$
$$\text{and} \quad x_1^2+x_2^2+x_3^2+x_4^2=17.$$

Thus
$$[a_{ij}]=\begin{bmatrix} 2x_1+1, & -1, & 0, & 0 \\ 0, & 2x_2+1, & -1, & 0 \\ 0, & 0, & 2x_3+1, & -1 \\ 2x_1, & 2x_2, & 2x_3, & 2x_4 \end{bmatrix}$$

It is required to find the solution of these equations, to 5 decimals, in the neighbourhood of $x^{(1)}=(0{\cdot}6, 0, -3{\cdot}6, 1{\cdot}5)$. We have $a^{(1)}=a_i(x_j^{(1)})=(-0{\cdot}04, -0{\cdot}40, -1{\cdot}14, -1{\cdot}43)$, while

$$A_1=\begin{bmatrix} 2{\cdot}2, & -1, & 0, & 0 \\ 0, & 1, & -1, & 0 \\ 0, & 0, & -6{\cdot}2, & -1 \\ 1{\cdot}2, & 0, & -7{\cdot}2, & 3 \end{bmatrix}$$

$$A_1^{-1}=\begin{bmatrix} 0{\cdot}464363, & 0{\cdot}464363, & -0{\cdot}053996, & -0{\cdot}017999 \\ 0{\cdot}016293, & 0{\cdot}758170, & -0{\cdot}083369, & -0{\cdot}028060 \\ 0{\cdot}021598, & 0{\cdot}021598, & -0{\cdot}118791, & -0{\cdot}039597 \\ -0{\cdot}133909, & -0{\cdot}133909, & -0{\cdot}263499, & 0{\cdot}245500 \end{bmatrix}$$

Hence

$x^{(2)}-x^{(1)}=-A_1^{-1}a^{(1)}=(0{\cdot}117, 0{\cdot}217, -0{\cdot}183, -0{\cdot}008)$ to 3 decimals,

$$x^{(2)}=(0{\cdot}717, 0{\cdot}217, -3{\cdot}783, 1{\cdot}492),$$
$$a^{(2)}=(0{\cdot}01409, 0{\cdot}04709, 0{\cdot}03609, 0{\cdot}09833),$$
$$x^{(3)}-x^{(2)}=-A_1^{-1}a^{(2)}=(-0{\cdot}02469, -0{\cdot}04023, 0{\cdot}00686, -0{\cdot}00644),$$
$$x^{(3)}=(0{\cdot}69231, 0{\cdot}17677, -3{\cdot}77614, 1{\cdot}48556),$$
$$a^{(3)}=(-0{\cdot}00517, -0{\cdot}01584, -0{\cdot}00247, -0{\cdot}02334).$$

Comparison of $a^{(3)}$ and $a^{(2)}$ suggests that it would probably take five or six more iterations to produce the required result. Even this would probably be quicker, in general, than the reciprocation of the matrix A_3 in order to improve the rate of convergence. In the present instance, however, the A matrices are comparatively simple to reciprocate, and the effect of replacing A_1^{-1} by A_3^{-1} is readily demonstrated. Thus we have

$$A_3=\begin{bmatrix} 2{\cdot}38462, & -1, & 0, & 0 \\ 0, & 1{\cdot}35354, & -1, & 0 \\ 0, & 0, & -6{\cdot}55228, & -1 \\ 1{\cdot}38462, & 0{\cdot}35354, & -7{\cdot}55228, & 2{\cdot}97112 \end{bmatrix}$$

$$A_3^{-1}=\begin{bmatrix} 0{\cdot}426186, & 0{\cdot}317942, & -0{\cdot}034961, & -0{\cdot}011767 \\ 0{\cdot}016293, & 0{\cdot}758170, & -0{\cdot}083369, & -0{\cdot}028060 \\ 0{\cdot}022053, & 0{\cdot}026213, & -0{\cdot}112843, & -0{\cdot}037980 \\ -0{\cdot}144497, & -0{\cdot}171755, & -0{\cdot}260622, & 0{\cdot}248855 \end{bmatrix}$$

$x^{(4)}-x^{(3)}=-A_3^{-1}a^{(3)}=(0{\cdot}00688, 0{\cdot}01123, -0{\cdot}00064, 0{\cdot}00170),$

$$x^{(4)}=(0{\cdot}69919, 0{\cdot}18800, -3{\cdot}77678, 1{\cdot}48726),$$
$$a^{(4)}=(0{\cdot}000057, 0{\cdot}000124, 0{\cdot}000027, 0{\cdot}000220),$$
$$x^{(5)}-x^{(4)}=-A_3^{-1}a^{(4)}=(-0{\cdot}000060, -0{\cdot}000087, 0{\cdot}000007, -0{\cdot}000018),$$
$$x^{(5)}=(0{\cdot}69913, 0{\cdot}18791, -3{\cdot}77677, 1{\cdot}48724)$$ to 5 decimals,
$$a^{(5)}=(0{\cdot}0000027, -0{\cdot}0000098, -0{\cdot}0000184, -0{\cdot}0000326).$$

5. A set of n equations in the complex quantities z_j $(j=1, 2, 3, \ldots n)$ is equivalent to a set of $2n$ equations in the real quantities x_j and y_j, where $z_j=x_j+\sqrt{(-1)}y_j$, since the functions $a_i(z_j)$ when expanded take the form

$$b_i(x_j, y_j)+\sqrt{(-1)}\,c_i(x_j, y_j)$$

and hence
$$b_i(x_j, y_j)=c_i(x_j, y_j)=0.$$

If the coefficients of $a_i(z_j)$ are real, the roots are in conjugate complex pairs. The evaluation of such expressions as $a_i^{(1)}$ and $a_{ij}^{(1)}$ is often most easily carried out by division by the corresponding real quadratic factors, rather than by direct substitution.

For example, suppose

$$a_1(z_j)=z_1^3z_2+2z_1^2z_2-z_1z_2^3+3z_1z_2+16,$$

and that $z_1^{(1)}=1\pm\sqrt{(-1)}$ and $z_2^{(1)}=-1\pm2\sqrt{(-1)}$.

The approximate real quadratic factors are thus

$$Q_1=z_1^2-2z_1+2 \text{ and } Q_2=z_2^2+2z_2+5.$$

We now divide $a_1(z_j)$, by the usual process of algebraic division, by Q_1, and the remainder by Q_2. In this way we find that

$$a_1(z_j)=(z_1z_2+4z_2)Q_1+(2z_1-z_1z_2)Q_2+ +(10z_1z_2-10z_1-8z_2+16).$$

Hence

$$a_1^{(1)}=10z_1^{(1)}z_2^{(1)}-10z_1^{(1)}-8z_2^{(1)}+16$$
$$=10(-3\pm\sqrt{(-1)})-2\pm26\sqrt{(-1)}+16$$
$$=-16\pm16\sqrt{(-1)}.$$

The extension to any number of unknowns is obvious. The process is of course only applicable when the $a_i(z_j)$ terms are polynomials.

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CHESTER BRANCH AERONAUTICAL SHOW AND GARDEN PARTY 1958

THE CHESTER BRANCH is particularly fortunate in having in its area the de Havilland factory sited in one of the most beautiful parts of Cheshire and, at that factory, a benevolent management which, as soon as the question of a garden party was raised, promptly offered the use of their sports ground for the event. The idea met with great enthusiasm from R.A.F. Hawarden who occupy the adjoining airfield and, after the usual flow of paper work, permission was obtained to use it for a flying display. A lot of work on the part of the organising committee ensured an interesting programme for the afternoon, and by the time the first visitors arrived on 31st May 1958 the only thing in doubt appeared to be the weather. Even this, however, settled down to a fine afternoon without rain.

The visiting V.I.P.'s had been entertained to lunch by Mr. J. C. Corby, the Branch President and Manager of the de Havilland factory. When they arrived on the field a considerable crowd had already collected to witness the opening ceremony. This was performed by Sir Arnold Hall, President the Royal Aeronautical Society, who said that he was very glad to find that he was not expected to mark the occasion by a spectacular entry by parachute, and went on to welcome the visitors to the first function of this nature organised by a provincial branch of the Society. Sir Arnold was introduced by Mr. J. C. Corby supported by Mr. J. G. Sharp, the Chester Branch Chairman.

While Sir Arnold was making his closing remarks, the English Electric P.1 made a few low level runs over the airfield, thereby starting the flying display. The programme of flying which followed was more reminiscent of the pre-war Hendon days than post-war Farnborough. The R.A.F. contingent provided the more modern touch with the Vampire T.11, the Beverley and the Whirlwind helicopter which really showed its paces. The Swordfish and the Mosquito provided for those enthusiasts who like the nostalgic touch at their displays. The A.T.C. Sedburgh gliders gave demonstrations of towed and winched-off flights. The other light aircraft taking part included a Piper Tripacer, which the Marquess of Kildare had flown from Ireland despite some servicing troubles and bad weather, a Foster Wikner-Wicko, the only one in existence, a Fairey Junior and a Leopard Moth. A Beechcraft Bonanza was used to set the problem for a height and speed competition. Special mention should be made of the fine display by a team consisting of an Auster Alpha, Auster Autocrat and Tipsy Belfair, which was provided by the Lancashire Aero Club.

Sir Arnold Hall, President of the Society performing the opening ceremony at the Chester Branch Aeronautical Show and Garden Party. On his left is Mr. J. C. Corby, President of the Branch and on his right Mr. J. G. Sharp, Chairman of the Branch.

In two large marquees, twenty-two exhibitors put on a show which provided a great deal of interest. To the small boys, one of the greatest attractions was the "intercom" system where one could don earphones and microphone and carry on a conversation, thickly interspersed with "Roger" and "Over," to a friend about five yards away. This miniature Farnborough show included aircraft models, the wardrobe and log book of Alcock and Brown, and accessories varying from a hydraulic valve to a jet engine.

But after all, this was a party, and children had been invited, so the family men of the committee had been consulted and had decided that to keep children amused the simplest devices were generally the best. The R.A.F. therefore provided an old Hurricane which was left entirely to the tender mercies of the children. Those who could not get into the cockpit to "fly" it, slid down the wings. The total casualties reported were one pair of trousers caught on a protruding rivet and a slightly bruised hand which was trapped while investigating the mechanism of an aileron. The A.T.C. supervised the use of two gun turrets which were never short of occupants. The Punch and Judy Show drew the usual crowd of children and adults to its performances. The Buckley Town Band provided a musical selection before and after the flying display.

Drinks and refreshments were obtainable on the field and teas were provided in the de Havilland canteen.

The fine weather relieved the committee of the necessity of a last minute switch to the alternative wet weather programme.

A general impression of the afternoon was that the 1,200 people present were enjoying themselves. Perhaps special mention could be made of the support of the Manchester Branch whose members arrived in large numbers, the party of school boys who came 60 miles from Lytham St. Annes, the Royal Observer Corps party of over 100 strong, and the enterprising Junior Women's Air Corps programme sellers who (by methods which we did not investigate) managed to hand in the money for more programmes than had been printed!

Final proof of the success of the whole show is that the Branch has gained 20 new members.—R.G.H.

Graduates' and Students' Section

Free refreshments at lectures

In response to an appeal at the A.G.M. for more opportunity for making social contacts within the Section we are arranging for free coffee and biscuits to be made available in the half hour before lectures during the Autumn session. If this is a success (and we are not bankrupted in the process) we will make it a regular feature.

Poster and ticket design competition

We invite designs for a poster and ticket for the Section's dance on 28th November. There will be two free tickets for the winner in each category. Size of the poster can be 9 in. × 13 in., or × 15 in. and the ticket 3 in. × 4½ in. Entries, which will not be acknowledged, should be sent to the Hon. Editor, P. T. Ross, 16 St. George's Road, St. Margarets, Twickenham, Middlesex.

Representatives

Two more people have offered to act as representatives, and members living in their areas should make complaints and suggestions to them for onward transmission to the Committee.

Mr. D. Lennard, c/o Royal Naval College, Greenwich, S.E.10.

Mr. A. N. Gobby, B.Sc.(Eng.), 90 Elmwood Crescent, Luton, Beds.

Farnborough Visit

On the 23rd June, we paid a return visit to the R.A.E., Farnborough.

After a brief explanation on the working principles of a shock tube and hypersonic wind tunnel, we were promptly transferred to the far side of the airfield where both types were inspected.

With the lunch hour behind us, our next visit was to the very impressive transonic wind tunnel, which we were able to inspect so thoroughly that it was possible to stroll down the working section.

Leaving the wind tunnel we then proceeded to the laboratory concerned with the development and testing of the Skylark research rocket. Here the cloak of security was temporarily raised and we were able to see the various forms in which this rocket will soon be tested in conjunction with the International Geophysical Year.

After a short visit to the structures laboratory, we proceeded to the model workshops, where the various shapes and forms to be observed gave rise to much thought on what types of aircraft will soon be darkening our skies.—B.G.

Visits

Visits have been arranged to two newspapers in September. These will be treated as one visit but applicants should state their preference. If either visit is over-subscribed names will be transferred to the other.

Friday 19th September, *Daily Telegraph*, 9.15-10.45 p.m., 10 people.

Tuesday 23rd September, *Daily Express*, 9·00-11.00 p.m., 12 people.

Applications should be addressed to the Hon. Visits Secretary, Mr. A. R. M. Pickering, Riverside, Vicarage Walk, Bray, Berks.

Gatwick Visit

The first thing which struck visitors to Gatwick Airport on Wednesday 16th July was the elimination of the airport bus. Passengers arrive by rail or private car and enter the departure hall on foot at second floor level. Here we were shown how the passenger books in at any one of a series of desks and surrenders his baggage which disappears immediately on a conveyer belt, passing briefly before the gaze of Customs, who select a piece here, and a piece there, at random. It then goes down to ground floor level and is loaded onto the baggage loading vans.

The passenger makes his way downstairs to the first floor either by the Domestic or Customs stairway and walks along the finger (which the M.T.C.A. prefer to describe as a pier) until he reaches the appropriate gate opening onto the aircraft stand. Each pier is divided down the centre at first floor level, Customs on one side, and Domestic, or non-Customs, on the other, and exit to the stand is made via further stairs to ground level and out through separate doors. By selecting the right doors a Customs or non-Customs flight may be cleared from the same aircraft stand.

Two more piers are to be built, and the main bulding doubled in size as the demand grows. Everywhere is seen evidence of lessons learnt at London Airport being applied, with flexibility foremost in the planner's mind.

The visit concluded with a visit to the control tower which is at present out of sight behind trees, over a mile from the terminal building, and we heard of plans for another runway, parallel to the first, leaving the tower in the centre.

We came away feeling that if not at present large, Gatwick technically bears comparison with any European airport, and the field for future expansion is wide open.—P.T.R.

Gatwick Airport from the air. A photograph taken on the opening day showing the terminal building, railway station, extension pier (with parked aircraft), and the 7,000 ft. runway.

(Photograph by courtesy of British European Airways)

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Reviews

EVIDENCE IN CAMERA: The Story of Photographic Intelligence in World War II. *Constance Babington Smith. Chatto and Windus, 1958. 252 pp. 18s.*

This book makes many appeals and holds many qualities and interests. First of all it is the excellently written record of the science-cum-art of photo interpretation told by one of its leading exponents. As such it has many of the qualities of a high-brow thriller, and no part is more absorbing than the piecing together of the V.1 puzzle in which the author took such a distinguished part. Then there is the unfolding of new techniques in a comparatively new vocation; the advantage of turning the photographs before study so that the shadows fall towards the examiner; the division of examination into first, second and third phases to enable the maximum short and long-term action to be taken on the photographs; the growingly realised importance of regularly covering the same location; and the extraordinary familiarity built up with the enemy's habits and thoughts, as when the interpreters could tell from the Germans' way of parking their aircraft whether Luftwaffe or factory pilots were involved. Skill and ingenuity were called out in the most curious and fruitful ways, when for example a formula was worked out for telling the speed of a ship from the wave patterns in its wake, and when a completely new technique was evolved to interpret night photographs.

Running right through the book is also the part-and-parcel story of the pilots and aircraft which produced the material on which the interpreters worked, a story of heroism and devotion as high as any in the air war, with the pilots armed only with cameras, and protected only by human daring and ingenuity. Brought clearly out, too, is the inspired amateurism that started the whole business of photographic reconnaissance and interpretation, and the equally inspired professionalism that carried it on and through to the end.

Not the least interesting to this reviewer is the fascinating parade of professions which went to make up the expert interpreters, from airmen to archaeologists, and the gratifying fact that women proved every bit as expert as the men in this sub-world of war. Finally a word of tribute must be paid to the delightful way in which the author has told her story, and the many touches that bring alive the participants. When Miss Babington Smith has narrated the ragging which greeted Frank Whittle after the time he had spent with her one day at Medmenham, she adds a footnote to answer one of the questions the Staff College put to Whittle: what perfume was she wearing? It was Guerlain's *L'Heure Bleue*.—CHARLES GIBBS-SMITH.

THE FASTEST MAN ALIVE. *Lt. Col. Frank K. Everest, Jr. Cassell, London, 1958. 204 pp. Illustrated. 21s.*

This is "the personal life story of the world-famous American rocket plane pilot as told to John Guenther," another in the series of test pilot autobiographies.

Frank Everest was born in 1920 and at the age of ten saw his first aeroplane and "was aware of a new and strange desire to be up in the sky myself." Nothing developed, however, until at the age of 19 he joined the Army Air Corps cadets, abandoned teacher training and began to study engineering at West Virginia University. There he was able to learn to fly under the Government civilian training scheme and found himself fighting in Egypt in January 1943. After combat in Africa, Italy, China—mostly on the P.40 which Everest disliked—he was finally shot down and made a Japanese prisoner of war only three months before the Japanese surrender.

So far a very ordinary account of a life undistinguished from thousands of others. Everest decided to stay in the Army. "I wanted to keep on flying; what's more, I believed that aviation would grow." Again very undistinguished sentiments. The ten years from 1946-56 were, however, in a different category. Everest became a test pilot at Wright Field and later at Muroc (Edwards Air Force Base). Here he flew all the famous "hundred" series of jet fighters and, in the Super Sabre F100, obtained the last of the low altitude world speed records at 755 m.p.h.

Everest was particularly attracted to the rocket powered Bell aeroplanes X-1 and X-2. His account of the test flying of these is the best part of the book. They were dropped from B-29 and B-52 bombers and, in the course of a flight lasting less than 15 minutes, climbed to around 80,000 ft. and reached almost $M=3$. Later flights have done even better. The detailed description of the preparations for the drops, flights and landings occupy almost half of the book. The style is lurid ("The X-2 leaped forward convulsively, eating air, and I knew at once I could not hold it down. Today it hungered for the whole sky.") but the development troubles and the pilot tensions are well described.

The small number of flights which were entirely successful must have prepared those in the know for the disappointments of the later satellite launchings. The Bell aircraft rocket motors often either did not all fire, exploded or cut prematurely. At the end of each flight the pilot found himself some ten miles up in a supersonic glider. It is fortunate that the great, dry lake airfield was available for the landing. No pilot seems to have been unable to find the field, but the series of failures of the undercarriage and of uncontrollable landings at around 150 m.p.h. make depressing reading.

The fast flying (M more than 2) sounds really frightening. The X-1 and X-2 were unstable at these speeds and the damping of a disturbance was negligible. Yeager and Murray had got out of control in the X-1. "The aeroplane began tumbling through the stratosphere at terrible speeds, rolling and tossing and Yeager was thrown violently about in the cockpit at forces up to fourteen times the pull of gravity." After such experiences the descriptions by Col. Everest of his applying "pulses" to the controls at $M=2{\cdot}5$ to test stability are indeed tense.

Everest's story concerns little but his own activities. Countries other than America are scarcely mentioned. He made a visit to England to evaluate the Javelin and Hunter and comments that the Javelin had little instrumentation, was limited to $M=0{\cdot}93$ and was not a true delta because the sweep was 55° and not 60°! He liked the Hunter but thought them both rather slow. After flying at $M=3$, this is not surprising.—A. H. YATES.

CHEMISTRY PROBLEMS IN JET PROPULSION. *S. S. Penner. Pergamon, London, 1958 394 pp. Illustrated. 90s.*

This is the first of an international series of monographs which Pergamon Press, assisted by a team of

internationally known scientists who comprise the board of consulting editors, plan to publish over the next five years covering the entire field of Aeronautical Sciences and Controlled Flight. The Series is divided into a number of complementary divisions and a new book from each division will be published every six months. Each book will cover a specific subject in aeronautical science and controlled flight and will serve equally well as an authoritative review for the well-prepared beginner and a source book for the experienced worker.

The material in Chemistry Problems in Jet Propulsion is presented in three Parts—I Atomic and Molecular Structure, II Combustion Thermodynamics and III Combustion.

Part I is a brief (80 pages) but well-balanced introduction to the modern conception of the atom derived from the application of wave mechanics. It also includes relevant descriptions of chemical bonds (Chapter IV), standard heats of formation and bond energies (Chapter VI).

Part II deals in detail (130 pages) with the calculation of adiabatic flame temperatures and equilibrium compositions (Chapter XIII), performance of chemical propellants for rocket engines (Chapter XIV) and thermodynamic functions for ideal gases (Chapter XVI) which are preceded by a summary of the necessary basic thermodynamic principles.

Part III (175 pages) discusses chemical reactions in flow systems involving both aerodynamics and chemistry. Chapters XVII and XX giving an introduction to chemical kinetics will be familiar to the physical chemist who will only need to skim through them to familiarise himself with the author's mathematical notation. The chapters on diffusion flames (Chapter XXII), chemical reactions during expansion through a nozzle (Chapter XXIII), laminar flame propagation theory (Chapter XXIV), combustion scale-effect (Chapter XXV) and combustion processes in liquid-fuel rocket engines (Chapter XXVI) are excellent summaries of branches of current research that are of great technological importance. The original treatment of this section of the book provides ample evidence of the author's first hand familiarity with the subject matter.

This is a very readable text which was designed to be, and in fact is, suitable for university graduate students having either a chemical or an engineering background, though one feels that the contents of the book were selected with emphasis on the needs of the latter. Each chapter ends with a number of well-chosen problems and the reviewer believes that the monograph will serve the double purpose mentioned in the first paragraph.—B. P. MULLINS.

SIXTH SYMPOSIUM (INTERNATIONAL) ON COMBUSTION. *The Combustion Institute. Chapman and Hall, London, 1958. 924 pp. Illustrated. 224s.*

The Combustion Institute is an international scientific organisation the chief purpose of which is to organise Symposia. The sixth one was held at Yale in September 1956; the seventh is to be held at the end of August this year in Oxford.

The proceedings contain one hundred and twenty-five papers grouped under the headings: Structure and Propagation of Laminar Flames (37 papers), Structure and Propagation of Turbulent Flames (6 papers), High-Speed Reactions (8 papers), Flame Stabilisation in Fast Streams (12 papers), Instability in Combustion Chambers (4 papers), Ignition (5 papers), Combustion of Solid Fuels (8 papers), Combustion of Explosives and Solid Propellants (11 papers), Evaporation and Combustion of Droplets and Sprays (9 papers), Experimental and Analytical Techniques in Combustion (9 papers), Applications of Combustion (20 papers).

Since it is impossible to review such a large volume (over 900 pages) adequately in a short space, discussion will be confined to some of the themes most likely to interest the aeronautical scientist.

Boundary-layer theorists may find it worth-while to examine the papers on solid and liquid fuel combustion and on ignition processes at surfaces. The problems fit into two classes. In the first, exemplified by the burning of carbon and liquid hydrocarbons in air streams, the processes turn out to be very largely controlled by viscous action, thermal conduction, and diffusion; the techniques of boundary layer theory permit calculation of the burning rate. The chief difficulties are connected with the steep variations in fluid properties which result from the large temperature variations; but many combustion scientists are also hampered by inadequate familiarity with boundary layer theory. Their aeronautical colleagues could well lend a hand.

The second class of problems arises when either the fluid stream or the solid surface is not in thermodynamic equilibrium. The former case occurs when the fluid is a combustible gas mixture; the second arises in the "erosive" burning of solid rocket propellants past which the burned gases stream at high velocity. In such cases, the rate-constants of the chemical reaction have to be considered in addition to the heat and mass transfer processes; strong non-linearities are present. It would pay those aerodynamicists who are now becoming interested in the problems of very high-speed flight to take note of the progress achieved in this part of the combustion field; for problems have already been dealt with which are similar to those associated with the flow of dissociating gases adjacent to missiles re-entering the earth's atmosphere.

Combustion engineers are much concerned with turbulent flows, particularly those which occur in the immediate wakes of bluff bodies or in the mixing regions of gas jets. Such problems concern interactions between flow and combustion. Usually a grave difficulty is presented by the fact that inadequate knowledge is available about even the most elementary turbulent mixing processes when large density gradients are present. Aerodynamicists in search of new fields to conquer may find them here.—D. B. SPALDING.

INTO THIN AIR. *Dr. E. W. Still. Normalair Ltd., Yeovil. 1958. 215 pp. Illustrated. 18s. 6s.*

This is an easier book to recommend than to review. Undoubtedly it should be in the hands of all senior aircraft engineers and will give them a valuable and authoritative picture of the state of the art of aircraft crew and cabin pressure and air conditioning equipment. Its weakness lies in that it tends to be one sided, dealing, as a publication sponsored by a firm must, mainly with the firm's own products and lacking the balance that the presentation of different solutions can give. The author's firm and Dr. Still himself, however, are in the forefront of the subject and the reader is not likely to go far wrong in following the advice given.

But what a pity the Royal Aeronautical Society had not sponsored the book as one of its Monographs! Is it too much to hope that a future edition could be so published, thus enabling any omissions of content to be repaired?

The book starts off with a review of physiological factors in high altitude flight, clear enough but perhaps lacking in references to other literature covering a field

of some complexity. The non-medical reader may not fully appreciate the factors of human variability and that data given relate to good or required practice based on statistically probable human performance.

Chapter 2 is an essay on the general requirements for, and arguments around, the pressure systems; the next chapters deal with the systems and components of military and civil pressure cabins and pilots' pressure clothing, the various valves and controllers and air pressure generators of various types.

Chapter 5 is a particularly good discussion on heating and cooling problems, cycles and turbine and other heaters or coolers. Full chapters follow on temperature and humidity control systems and units and Chapter 8 deals similarly with air flow control.

Oxygen systems, stored gas and liquid types, are then dealt with for military and civil application, the latter relating U.S. requirements. The next chapter on mountaineering oxygen equipment is included for interest, although not of direct interest to aircraft engineers.

A comprehensive review of testing equipment, both for the main factory and for service checking of regulators and the like, is contained in Chapter 11; the final Chapter summarises M.O.S., A.R.B. and American design requirements.

The appendices are a little disappointing, that in Appendix 2 on duct losses having a useful chart for calculating losses but containing an incorrect and misleading mathematical summary of the derivation of an empirical formula.

Apart from this indication of lack of care with the manuscript, there are similar signs elsewhere: *e.g.* lb./min. in place of (lb./sq. in.)/min. in Chapter 1. Dr. Still should study the dictionary meaning of "protagonist," and he should remember that it is usual to have a verb in a sentence following a semi-colon (on p. 7 there is a sentence with two semi-colons and two verbs missing). And is not 50,000 ft. rather than 40,000 ft. the maximum safe altitude for short human exposures?

In spite of these minor criticisms the book is wholeheartedly recommended to all aircraft engineers and pilots. —H. G. CONWAY.

COMMERCIAL PILOT'S HANDBOOK, VOL. I. *Captain R. E. Gillman. Pitman, London, 1958. 227 pp. Illustrated. 25s.*

Captain Gillman has fulfilled the requisite of a good textbook: he has brought together the necessary information and has presented it well. That a textbook quite like this has not previously been produced is a measure of the task which Captain Gillman set himself.

The contents of this book are culled from a vast, scattered mass of documents, articles and books almost beyond the ability of an embryo pilot to collect and distil. Some have done it, of course, but the penalty suffered is unwanted knowledge and a great waste of time. Captain Gillman has streamlined and trimmed the information to essentials: this was the right approach. His second volume will amplify. This, too, is sound. His most difficult task will have been to decide what to omit. The difficulties here are great and will give rise to the main criticism of his book—not necessarily justified criticism. There are fifteen lines devoted to "Lapse Rate" and one sentence dealing with "super-cooled water drops." An examination question could well be:—"What do you understand by the term 'super-cooled water drops'?" Is one sentence enough? Are fifteen lines on "Lapse Rates" enough?

It is comforting to have a single concise book which contains a reference to every possible matter on which an examination question could be based. And, conversely, if "Tephigram" is not referred to in this book, one knows the examiners won't bother one with it. It is inevitable that the examination candidate will acquire books on meteorology, theory of flight, navigation, H.M.S.O. literature, and all the rest. If he wades through them all, it will take a long time. He will fill his head with a clutter of knowledge, much of which he does not need. Captain Gillman has produced a guide to essential knowledge. Used in this way, it will be a blessing.

Neither Captain Gillman's book nor the Commercial Licence will produce competent pilots. They aren't intended to do so. Both are directed towards minimum requirement—the first hurdle along the long rough path to high professional ability. Given an alert brain, there must be added the skill, special knowledge, and experience, which are the ingredients of the good professional pilot.

Captain Gillman's textbook was needed to help the beginner and as an *aide memoire* to the already qualified. It fulfils this need extremely well. And to those who are so frequently asked—"What book should I study?" there is, at last, this most welcome answer.—W. B. HOUSTON.

CAUSES AND PREVENTION OF CORROSION IN AIRCRAFT. *T. C. E. Tringham. Pitman, London, 1958. 124 pp. Illustrated. 25s.*

As the author states, "This book is written not as a treatise on corrosion, but to give engineers engaged on the construction and maintenance of aircraft an elementary knowledge of the subject and, with this enlightenment, a greater desire to prevent what is often referred to as the 'odd spot of rust.'"

This "odd spot of rust" can take the form of corrosion by chemical attack, electrochemical, intercrystalline, crevice, fretting and stress corrosion, corrosion fatigue and exfoliation. Each type of corrosion is described and methods to prevent its occurrence are recommended. Corrosion, with the exception of stress and fretting corrosion, can be prevented by using the correct anti-corrosive treatment and by ensuring during the life of the component that it is kept clean, and noting particularly that corrosion cannot take place unless an electrolyte is present; in pure dry air corrosion cannot occur at all. With stress corrosion, the prevention is in the design and assembly of the components; if one can prevent, by various means, residual stresses becoming locked up, then the problem is solved. Fretting corrosion is becoming more important in modern aircraft and its mechanism is only just being realised. This corrosion does not need an electrolyte and the author states that, on mating parts, even a minute movement can cause the surface of each part to roughen, and small particles are torn away which sift out and become oxidised, giving the tell-tale brown or black powder of steel and aluminium alloys. This type of corrosion is no doubt responsible for some fatigue failures; molybdenum disulphide is recommended as a preventative.

Discussion of the mechanism of each type of corrosion is very brief and, in particular, fretting and stress corrosion have only been given an elementary introduction. Other aspects of corrosion included are:—"Aircraft in Service," "Corrosion in Electrical Equipment," and "Causes and Prevention of Corrosion in Engines and the Associated Components," which deals with the problems of leaded fuels with regard to engine interiors and the corrosion of fuel tanks and exhaust systems.

Part of the book deals in some detail with various processes designed to prevent corrosion under the heading "The Electroplating Process"; cadmium plating is thoroughly dealt with, zinc plating is also reviewed. A chapter

deals with the anodic oxidation of aluminium and chromating of magnesium, anodic treatment by chromic and sulphuric acids and chromating magnesium being dealt with in detail. "The Metallising Process," "Pre-treatments of Metal Prior to Painting" and "Non-destructive Examination," all receive their allotted space.

The book gives a good introduction to corrosion and its associated problems and should be of interest to all engaged in the manufacture and maintenance of aircraft.—T. R. ASHCROFT.

DYNAMICAL AND PHYSICAL METEOROLOGY. *George J. Haltiner and Frank L. Martin. McGraw Hill, London, 1958. 470 pp. Illustrated. 75s.*

Every meteorologist has his own ideas about what are the important branches of the subject. Unless the authors have been in the forefront in developing some part of it, the treatment tends to be orthodox. When faced with new developments, they attempt to present the ideas of provocative writers as faithfully as they can, but are scarcely in a position to guide readers by telling them what to notice and what to ignore in the new work. Doctors Haltiner and Martin (Professors at the U.S. Naval Postgraduate School) are, therefore, inclined to write what they feel safe about, and have produced another course book which is neither new in approach nor particularly authoritative. But, having decided to include a topic, they generally do it well.

On the whole, however, the treatment is suitable to the indoor student of meteorology who has no advanced mathematics or physics available. For this reason, and because it does not call on any out-of-doors experience, the book does not go far enough with some of the most recent ideas which are included. Also many old cobwebs remain. Thus, the Chapter on "the mechanism of pressure change" precedes that on "vorticity and circulation." If it were to follow, and full use were made of the modern ideas of vorticity, it would be seen that the former is unprofitable.

The treatment of fronts is old-fashioned and the rôle of the jet stream is almost ignored in the whole book. Indeed, were it not for the Chapters on diffusion of wind structure in the friction layer, diffusion, and atmospheric pollutants, one would suspect that the authors made no effort to connect their patterns of isobars with what they see outside. For them clouds scarcely exist and this is not because they do not come within the bounds of their title but because they have preferred to fill their book with other things.

The Tephigram (used in all British and many other meteorological offices) is not mentioned by name and one suspects that this is because the authors have managed to give courses without it for several years. The other good diagram (the skew $T-\log p$) used by meteorologists is briefly described in a rather muddled way, and its advantages are evidently either not understood or not believed in by the authors. For instance, there is no discussion of how to calculate upper air thickness patterns —which every meteorologist ought to be able to do.

We find, therefore, just another course book with nothing special to recommend it. Indeed, those with aeronautical interests should be positively warned against it because it will seem to them to treat quite another medium from that in which they fly—namely, the stuffy atmosphere of the blackboard and examination room.—R. S. SCORER.

ANALYTICAL DESIGN OF LINEAR FEEDBACK CONTROLS. *G. C. Newton, L. A. Gould and J. F. Kaiser. Chapman and Hall, London, 1958. 418 pp. Illustrated. 96s.*

As its title suggests this book is really concerned with the design of servo-mechanisms and is not intended or suitable as an introduction to the theory and principles of the subject. The three authors are all in the Electrical Engineering department of the Massachusetts Institute of Technology and have had considerable experience, not only in actual research and design, but in teaching this aspect of automatic control. The book is of high scholarship and high academic standard and is vital to those primarily engaged in logical design. Some familiarity with simple linear theory is assumed, but where the analysis requires mathematics not likely to be generally familiar to engineers no previous knowledge is required. The authors have taken great care to introduce new mathematical techniques from a simple basis. A particular example of this is Chapter Three, which forms an excellent introduction to that branch of statistical analysis required for the statistical design procedure dealt with at length in further chapters. An introduction to the meaning and importance of stochastic processes is given, followed by considerations of probability-density functions and the implications of the ergodic hypothesis for the use of stationary time series in servo systems. The chapter continues with the derivation of correlation functions and some explicit examples worked out in detail.

The main thesis of the book is relatively new work and consists in design by variation of certain parameters to obtain optimisation. Two forms are considered: optimisation of performance to step function inputs and optimisation to the stochastic behaviour when subjected to a continuously varying random signal of given stationary statistical properties.

A criticism of previous design methods for linear systems, that in fact real systems are always non-linear especially owing to saturation, has been met by placing the parametric variations necessary to take place for optimisation under a form of constraint. This constraint is formulated so that the probability of the system saturating at the load point is kept to a significantly low figure.

The book ends with six appendices, one of which is a most valuable table of integrals of the form used frequently throughout the book. This table for this series of integrals is the most comprehensive yet published.—J. C. WEST.

MY ZEPPELINS. *Hugo Eckener. Putnam, London, 1958. 216 pp. Illustrated. 21s.*

This is the story of Eckener's own particular airships, the Graf Zeppelin and Hindenburg, written by the master himself.

In an Introduction he tells of his first meeting with Count Zeppelin, of which there have been many versions, and how he, a philosopher and political economist, fell under the spell of Zeppelin and became an airship captain.

Eckener regrets that Captain Strasser, "Leader of Naval Airships" had not held that position two or three years before the war. They would then have had airships of outstanding performance, operating safely at higher altitudes, and the air raids on London "could have had an effect hardly to be over estimated." Eckener considers this "was a tragedy of lost opportunity."

Peter Strasser was Eckener's hero and his opinion is therefore somewhat coloured. There is no reason to suppose Strasser's earlier arrival in airships would have done any more than Count Zeppelin or Eckener himself. The zeppelin, like the aeroplane, developed with the war.

After the war the Germans destroyed some of the zeppelins that were to be handed over to the Allies. In the first chapter Eckener claims that the Zeppelin Company

proposed to pay America's share of compensation for the loss of the destroyed zeppelins by building one for them. The Company was then in no position to make such a proposal. It was Mr. Lloyd George, the Prime Minister, who first suggested such a ship for America, and so saved the zeppelin works for Eckener. He makes no mention of this help from the British, at what was a critical time for his Company. The new zeppelin was the Z.R.11 (Los Angeles) and in taking her to America Eckener gained valuable experience for his future operations.

We have the story of how he and his staff toured Germany talking airships, passing the hat round, and collecting the money to build the Graf Zeppelin. The Graf Zeppelin flights make fascinating and instructive reading. Here is Eckener at his best, the supreme airship pilot and weather man. Properly constructed and piloted there was nothing wrong with the airship as a means of transportation. Describing the Arctic flight, he makes a scathing attack on General Nobile, calls him a "skittish colt," the "weeping and hand wringing Nobile," and says that he refused Nobile's offer to accompany him as a "Polar expert," Eckener having no wish "to accumulate experiences on a cake of ice." This is very surprising when one remembers that in that same year, 1931, Eckener asked Nobile to join him in organising a regular airship service to South America. Eckener does not mention this, and one wonders why the change of heart.

While under the cloud of his "impaired political health" he learned of the loss of the Hindenburg. Eckener agrees with the findings of the court of investigation, and says this finished the rumour of sabotage. But survivors of the crew, and experienced airship eye witnesses on the ground, are still of the opinion is was sabotage.

The book concludes with a chapter by Eckener's son Knut, giving most interesting details of zeppelin construction, performances, and problems overcome.

There are a few minor mistakes, and one can only imagine that at times Eckener, over 80, was writing from memory. The photograph caption "Captain of the Deutschland II" is an error. This is the Control car of the Schwaben. A word of explanation may be necessary about the phrase "on flank speed" used on p. 113, which abruptly reminds the reader that the translator is American. The English equivalent is "on full speed."

This is a book to do much to disprove the adverse criticism and opinion of airships.—CAPTAIN J. A. SINCLAIR.

FLYING TALES FROM BLACKWOOD. *William Blackwood. Edinburgh and London, 1957. 268 pp. 15s.*

Blackwood's Magazine is one hundred and forty years old. Known to its friends as "*Maga*," it has set a standard of good prose and good taste which is unsurpassed. To be accepted by Blackwood is an enviable achievement for a storyteller. Work or play, humour or drama, war or peace, all are to be found in this famous publication.

During the past twenty-seven years, many flying stories have appeared in "*Maga*" and a collection of seventeen of the best of them, covering fifty years of flight, has now been published with a foreword by Air Chief Marshal Sir Dermot A. Boyle, Chief of the Air Staff. This is a really first-class selection of stories which are intensely interesting —some humourous, some dramatic, some moving but all written in a fast-moving style.

Significantly, there is not a serious error, technical or otherwise, to be found in the book. So often, in magazines which do not specialise in aeronautical matters, the mention of an aeroplane brings with it a host of irritating errors which quite destroy the atmosphere or break the spell of an otherwise absorbing adventure. Not so with Blackwood, which proves that it can cater for a specialised readership with confidence and cheerful dignity.—A.S.C.L.

Additions to the Library

Automation in Practice. S. E. Rusinoff. American Technical Society. Technical Press, London. 1957. 261 pp. Illustrated. 55s. The emphasis is on the *application* of automation in various manufacturing and metal working processes. The first five chapters discuss the basic principles of automatic control in so far as they are relevant to manufacturing processes; the next eight consider the application to particular processes such as metal working, metal cutting, pressworking, joining, heat treating, casting, and the production of ferrous and non-ferrous metals; the last chapter deals with automatic inspection and quality control.

Autoren-Verzeichnis zur Bibliographie der Veroffentlichungen uber den Leichtbau und seine Randgebiete im deutschen und auslandischen Schrifttum aus den Jahren 1940 bis 1954. H. Winter. Springer-Verlag, Berlin. 1957. 103 pp. DM 8.00. An author Index for this invaluable bibliography, reviewed in the February 1957 JOURNAL, further enhances its usefulness.

Behind the Sputniks. F. J. Krieger. Public Affairs Press, Washington. 1958. 376 pp. $6. *To be reviewed.*

Digital Computer Programming. D. D. McCracken. John Wiley, New York (London—Chapman and Hall). 1957. 253 pp. Illustrated. 62s. *To be reviewed.*

Elements of Classical Thermodynamics. A. B. Pippard. Cambridge University Press. 1957. 164 pp. Illustrated. 15s. *To be reviewed.*

Flying Matilda. Norman Ellison. Angus and Robertson, London. 1958. 340 pp. 25s. *To be reviewed.*

Glossary of Terms used in High Vacuum Technology. B.S. 2951. British Standards Institution, London. 1958. 29 pp. 7s. 6d.

Guided Missiles. U.S. Dept. of the Air Force. McGraw-Hill, London. 1958. 575 pp. 62s. *To be reviewed.*

Inventors of Our World. Joachim Leithauser. Weidenfeld and Nicolson, London. 1958. 257 pp. 25s. A history, in non-technical language, of the technological revolutions which have produced our modern world, with emphasis on personalities rather than technicalities, and, with a chapter on the history of flight.

Pathfinder. Air Vice-Marshal D. C. T. Bennett. Frederick Muller, London. 1958. 287 pp. 18s. *To be reviewed.*

Vibration and Impact. Ralph Burton. Addison-Wesley, Massachusetts. 1958. 310 pp. *To be reviewed.*

War in the Air. Gerald Bowman. Pan Books, London. 1958. 176 pp. 2s. 6d. This is the first instance that the Library has had of receiving a "paperback" edition of an "orthodox" book that has already been reviewed (page 811 JOURNAL December 1956). One might not spend 16s. on the original but nobody will grudge 2s. 6d. for a reminder of the time when it was felt that an air force was officially recognised as being of some use.

16th December 1919. Sir Hudson Fysh. Royal Aeronautical Society, Australia Division, Adelaide Branch. 1958. 13 pp. The inaugural Sir Ross and Sir Keith Smith Memorial Lecture, dealing with the background and achievement of what Sir Hudson calls "the Greatest date of all in the history of Australian Civil Aviation.

Reports

AERODYNAMICS

See METEOROLOGY

BOUNDARY LAYER—*see also* THERMO-AERODYNAMICS

Measurement of the effect of an axial magnetic field on the Reynolds number of transition in mercury flowing through a glass tube. M. Bader and W. C. A. Carlson. N.A.C.A. T.N. 4274. *May* 1958.
Experiments were conducted to determine the effect of a 15,000 gauss axial magnetic field on the flow of mercury through a glass tube of 0·027 in. inside diameter. It was found that when only slight instabilities were present at Reynolds numbers from 5,000 to 8,000, the Reynolds number of transition could be increased by as much as 10 per cent.—(1.1.2.1).

Effects of fixing boundary-layer transition for an unswept-wing model and an evaluation of porous tunnel-wall interference for Mach numbers from 0·60 to 1·40. L. S. Stivers and G. W. Lippmann. N.A.C.A. T.N. 4228. *April* 1958.
Tests have been made in the Ames 2 ft. × 2 ft. transonic wind tunnel to determine the aerodynamic effects of fixing boundary-layer transition in a forward location on two unswept-wing models which differed only in size. The models had wings of aspect ratio 3·09 with sharp leading edges. The tests were made at Reynolds numbers from 1·5 to 3·45 million.—(1.1.2.3 × 1.12.1.2).

Observations of turbulent-burst geometry and growth in supersonic flow. C. S. James. N.A.C.A. T.N. 4235. *April* 1958.
Some aspects of the mechanics of boundary-layer transition have been deduced from a study of shadowgraphs of free-flight models at free-stream Mach numbers between 2·7 and 10.—(1.1.2.4).

COMPRESSIBLE FLOW

Pressure distributions at transonic speeds for parabolic-arc bodies of revolution having fineness ratios of 10, 12 *and* 14. *R. A. Taylor and J. B. McDevitt. N.A.C.A. T.N.* 4234. *March* 1958.
The measured static-pressure distributions at the model surface and in the surrounding flow field are presented for the bodies at zero angle of attack. The Mach number range extended from 0·80 to 1·20 and the Reynolds number range was $23{\cdot}4 \times 10^6$ to $28{\cdot}6 \times 10^6$ (based on the theoretical length of the model).—(1.2.2 × 1.6.1).

A performance analysis of method for handling excess inlet flow at supersonic speeds. D. P. Hearth and J. F. Connors. N.A.C.A. T.N. 4270. *May* 1958.
For the range $M = 1{\cdot}5$ to $M = 4$, the following schemes were considered: normal and oblique shock spillage, by-pass through an auxiliary exit, by-pass to an ejector nozzle, and by-pass through an auxiliary ram-jet engine. These methods were evaluated in terms of a thrust penalty for an assumed turbo-jet engine. Charts are presented for estimating these penalties for a range of mass flows and Mach numbers. Comparison of the various methods is also made on the basis of matching requirements for a hypothetical $M = 4{\cdot}0$ turbo-jet application.—(1.2.3.1 × 27.1).

Magneto-gasdynamics of hypersonic Couette flow. Z. O. Bleviss. Douglas Report No. S.M. 23098. *February* 1958.
Couette flow problem is extended to include a uniform externally imposed magnetic field normal to the walls. Assuming thermodynamic equilibrium and reasonable variations of electrical conductivity, viscosity, and Prandtl number with temperature, numerical solutions are presented for air for the case of the insulated wall (no heat transfer to the wall) for Mach numbers from 10 to 30 and for the case of heat transfer for Mach numbers of 20 and 30. The effects of the magnetic field upon the velocity, temperature, electrical current density, and induced magnetic field distributions and upon the skin friction, heat transfer, and total drag (*i.e.* skin friction plus the magnetic stress due to the interaction of the electrically conducting fluid with the magnetic field) are shown. The expected qualitative and quantitative applicability of the Couette flow results to the boundary layer are discussed.—(1.2.3.1).

Blast from a pressurised sphere. D. W. Boyer et al. U.T.I.A. Report No. 48. *January* 1958.
Some experimental results are presented of the spherical flow generated by the shattering of 1, 2 and 5 in. diameter glass spheres initially filled with air, helium, or sulphur hexafluoride at pressures up to 22 atmospheres. Radius-time plane drum-camera schlieren photographs are presented of the blasts from all spheres as well as time sequence spark-shadowgraph pictures of the blast from the 2 in. diameter air spheres.—(1.2.3.2).

CONTROL SURFACES—*see* WINGS AND AEROFOILS

INTERNAL FLOW—*see also* METEOROLOGY

Maximum theoretical tangential velocity component possible from straight-back converging and converging-diverging stators at supercritical pressure ratios. T. P. Moffitt. N.A.C.A. T.N. 4271. *April* 1958.
An analytical investigation of the maximum theoretical tangential velocity component possible by expansion about straight-back stator blade trailing edges has been made by the method of characteristics for converging and converging-diverging stators.—(1.5).

Some actuator-disc theories for the flow of air through an axial turbo-machine. J. H. Horlock. R. and M. No. 3030. 1958.
Using actuator-disc theory, simplified methods are given for the solution of the direct problem of the incompressible flow of air through an axial-flow turbo-machine. The calculations are compared with other approximate solutions to the flow through a model compressor stage.—(1.5.3.1).

An approximate method for design or analysis of two-dimensional subsonic-flow passages. E. F. Valentine. N.A.C.A. T.N. 4241. *April* 1958.
A method is developed for the design and analysis of two-dimensional subsonic-flow passages with isentropic non-viscous flow. The method is based on the relation between the pressure change across a stream tube and the centrifugal force resulting from the curvature of the flow. Precomputed charts can be drawn which eliminate subsequent calculation for a given upstream Mach number. The method is limited by the accuracy with which the radius of curvature of the streamlines can be determined.—(1.5.1.2).

Summary of scale-model thrust-reverser investigation. J. H. Povolny et al. N.A.C.A. Report 1314. 1957.
The results of a scale-model thrust-reverser investigation are summarised. Three basic types of thrust reversers, target, tail-pipe cascade, and ring cascade, were investigated. All three were found to have satisfactory reverse-thrust performance and thrust-modulation characteristics.—(1.5.2).

LOADS—*see also* COMPRESSIBLE FLOW
WINGS AND AEROFOILS

Theoretical calculation of the power spectra of the rolling and yawing moments on a wing in random turbulence. J. M. Eggleston and F. W. Diederich. N.A.C.A. Report 1321. 1957.
An analytical solution is derived for the power spectral density of the rolling- and yawing-moment coefficients of a wing subject to random isotropic atmospheric turbulence. The theory considers the random variations of the turbulence in the spanwise as well as the flight-path direction. Horizontal, vertical, and side gust components are considered. Plots of the power spectra are presented.—(1.6.3 × 1.10.1.3 × 1.8.1.1).

STABILITY AND CONTROL—*see* LOADS
WINGS AND AEROFOILS
AEROELASTICITY

THERMO-AERODYNAMICS

Summary of experimental heat-transfer measurements in turbulent flow for Mach number range from 0·87 to 5·05. M. J. Brevoort and B. D. Arabian. N.A.C.A. T.N. 4248. *May* 1958.
An axially symmetric annular nozzle was used to obtain essentially flat plate data on turbulent heat-transfer coefficients and temperature-recovery factors. The results are for Mach numbers of 0·87, 1·62, 2·06, 3·03, 3·90, and 5·05 and for a Reynolds number range of 1×10^6 to $9{\cdot}5 \times 10^6$. These results are presented in the form of Stanton number and recovery factor as a function of Reynolds number.—(1.9.1).

Investigation of the laminar aerodynamic heat-transfer characteristics of a hemisphere-cylinder in the Langley 11-inch hypersonic tunnel at a Mach number of 6·8. D. H. Crawford and W. D. McCauley. N.A.C.A. Report 1323. 1957.
The aerodynamic heat transfer of a non-isothermal hemisphere-cylinder has been investigated at a Mach number of 6·8 and Reynolds numbers from $0{\cdot}14\times10^6$ to $1{\cdot}06\times10^6$ based on diameter and free-stream conditions.—(1.9.1×1.1.1.4).

WINGS AND AEROFOILS—*see also* LOADS

Calculation of the shape of a thin slender wing for a given load distribution and plan form. J. H. B. Smith. C.P. No. 385. 1958.
The problem of finding the camber and twist for a swept wing of specified slender plan form, which will produce the same chordwise lift distribution at each spanwise station as in two-dimensional flow past a flat plate, in conjunction with a uniform distribution of chord loading, is considered. Numerical results are given for the chordwise and spanwise variations of the slope of the wing surface measured in the chordwise direction, at several stations, and chordwise wing sections are drawn for two particular plan forms.—(1.10.1.2).

The geometry of wing surfaces generated by straight lines and with a high rate of thickness taper at the root. D. Peckham. C.P. No. 383. 1958.—(1.10.1.2).

Downwash tables for the calculation of aerodynamic forces on oscillating wings. Maths Division N.P.L. R. & M. 2956. *June* 1952.—(1.10.1.2×1.6.3).

Low-speed tests on 45-deg swept-back wings. Part 1. Pressure measurements on wings of aspect ratio 5. J. Weber and G. C. Brebner. Part II. Balance and pressure measurements on wings of different aspect ratios. D. Küchemann et al. R. & M. No. 2882. 1958.
The results of pressure measurements on three 45 degree swept-back wings with constant chord and aspect ratio 5, over an incidence range up to 10 degrees, are given. Chordwise and spanwise lift distributions are given, mostly near the centre where, on two of the wings, modifications had been made to the section shape.—(1.10.2.1×1.6.1).

Wind-tunnel interference effects on measurements of aerodynamic coefficients for oscillating aerofoils. W. P. Jones. R. & M. No. 2786. 1958.
A theory is developed for estimating the effect of wind-tunnel walls on measured values of aerodynamic coefficients for two-dimensional aerofoils oscillating in an incompressible fluid. An aerofoil describing translational and pitching oscillations in a wind tunnel of rectangular cross-section is considered.—(1.10.1.2×1.12.1.2).

Low-speed-tunnel model tests on the flow structure behind a delta-wing aircraft and a 40 degree swept-wing aircraft at high incidences. D. A. Kirby and A. Spence. R. & M. 3078. 1958.
In view of the possibility of trimming some swept-wing aircraft at incidences above the stall, there has been a desire to visualise the whole pattern of vortex sheets and separated flow starting from the stalling wing, and to follow it back beyond the tailplane. To supplement other methods, a swivelling head has been used, giving the velocity, pitch and yaw, and results are given for a 48 degree delta (Javelin) and a 40 degree swept-wing aircraft (Swift without fences).—(1.10.2.2×1.8.2).

General solutions for flow past slender cambered wings with swept trailing edges and calculation of additional loading due to control surfaces. E. B. Klunker and K. C. Harder. N.A.C.A. T.N. 4242. *May* 1958.
The slender-wing-type analysis is used to obtain general expressions for the surface pressure, lift, and rolling moment for cambered wings with swept trailing edges. Results are specialised to give the additional loading due to the deflection of trailing-edge control surfaces. The loading due to twist is also obtained for one particular form of twist.—(1.10.1.2×1.3.4).

On the lift and induced drag associated with large downwash angles. H. S. Ribner. U.T.I.A. T.N. No. 19. *January* 1958.
The lift and induced drag have been evaluated for an arbitrary wing-wake vortex pattern, with large downwash, *via* a momentum balance. The entire induction effect of the wake is found to be representable as a "generalised induced drag" vector parallel to the asymptotic trailing vortex direction. The vertical component of this drag constitutes a nonlinear contribution to lift. A possible similar behaviour of the parasite drag is indicated. The Jones low aspect ratio wing theory has been reformulated to apply to large angles of attack.—(1.10.1.2).

HELICOPTER AERODYNAMICS

Analysis of harmonic forces produced at hub by imbalances in helicopter rotor blades. M. Morduchow and A. Muzyka. N.A.C.A. T.N. 4226. *April* 1958.
General explicit expressions are derived for the harmonic forces produced at the hub by an *n*-bladed unbalanced helicopter rotor. Imbalances due to property differences among the blades and to non-uniform spacing between the blades are considered. Numerical examples are given throughout to illustrate the order of magnitude of the results. An analysis of the forces transmitted to the hub by a balanced rotor of *n* blades is also given.—(1.11.1).

Flight investigation of effects of retreating-blade stall on bending and torsional moments encountered by a helicopter rotor blade. L. H. Ludi. N.A.C.A. T.N. 4254. *May* 1958.
The results indicated that blade stall can produce large increases in periodic blade moments during high-speed flight and pull-up manoeuvres. The results also indicated that the higher harmonics can contribute as much as the lower harmonics to increased vibration and increased periodic control loads in the highly stalled condition.—(1.11.1).

Induced velocities near a lifting rotor with nonuniform disk loading. H. H. Heyson and S. Katzoff. N.A.C.A. Report 1319. 1957.
A method is given for converting known uniformly loaded loaded rotor induced velocities to correspond with arbitrary axisymmetric non-uniform disc load distributions. Numerical results for two specific distributions are given in chart form. Symmetry relations and relations between radial disc loading and wake velocities are developed. Experimental flow measurements are presented and compared with theory.—(1.11.1).

TESTING AND INSTRUMENTS—*see also* BOUNDARY LAYER
WINGS AND AEROFOILS

The calibration at transonic speeds of a Mk.9A Pitot static head with and without flow through the static slots. D. G. Mabey. C.P. No. 384. 1958.—(1.12.5).

AEROELASTICITY

See also STRUCTURES—THEORY AND ANALYSIS

Some measurements of aerodynamic forces and moments at subsonic speeds on a rectangular wing of aspect ratio 2 oscillating about the midchord. E. Widmayer et al. N.A.C.A. T.N. 4240. *May* 1958.
Results are presented for a range of Mach numbers from 0·15 to 0·81, a range of reduced frequency from 0·15 to 1·32, and a range of Reynolds number from $0{\cdot}60\times10^6$ to $9{\cdot}21\times10^6$. A comparison of the measured aerodynamic forces and moments is made with some available published data.—(2×1.8.2.2).

FLIGHT TESTING

See METEOROLOGY

HYDRODYNAMICS

An experimental investigation of wake effects on hydro-skis. E. E. McBride and L. J. Fisher. N.A.C.A. T.N. 4251. *May* 1958.
Two combinations of hydro-skis were tested: two hydro-skis in tandem and three hydro-skis arranged with a single front hydro-ski.—(17.2).

INSTRUMENTS

See METEOROLOGY

MATERIALS

A brief study of the suitability of titanium and a titanium alloy as firewall material. C. A. Hughes. C.A.A. T.D. Report 317. *September* 1957.
Tests were conducted on several grades of commercially pure titanium and one alloy to determine their strength at elevated temperatures and their resistance when exposed to a 2,000°F flame. Similar tests of stainless steel were conducted to provide a basis for comparison.—(21.2.2).

METEOROLOGY

Droplet impingement and ingestion by supersonic nose inlet in subsonic tunnel conditions. T. F. Gelder. N.A.C.A. T.N. 4268. *May* 1958.
The amount of water in cloud droplet form ingested by a full-scale supersonic nose inlet with conical centre body was measured. Local and total water impingement rates on the cowl and centre body surfaces were also obtained. All measurements were made with a dye-tracer technique. Inlet angles of attack of 0° and 4·2°, droplet diameters from 11 to 20 microns, and ratios of inlet to free stream velocity from 0·4 to 1·8 were studied. Measurements were confined to a free-stream Mach number of 0·237 and are extendable to other subsonic speeds by dimensionless impingement parameters.—(24 × 1.5.1).

Icing problems and recommended solutions. E. A. Brun. AGARDograph 16. *November* 1957. (*In English and French.*)
Various aspects of aircraft icing problems are discussed. Chapter headings are: 1. General Survey; 2. Icing Conditions to be considered in the Design of Protection Systems; 3. The Measurement of Icing Conditions; 4. Icing Wind Tunnel Tests; 5. Flight Tests in Simulated Icing Conditions; 6. Icing Flight Test Concepts in the U.S.A.—(24 × 1 × 13 × 18·4).

POWER PLANTS

See also AERODYNAMICS—COMPRESSIBLE FLOW

Transonic drag of several jet-noise suppressors. W. J. North. N.A.C.A. T.N. 4269. *April* 1958.
One-fifth scale suppressors were tested at Mach numbers from 0·65 to 1.10 over a range of nozzle pressure ratios. Total and component drag coefficients are presented for the various models. Drag increases due to the suppressors are presented in terms of net thrust. Propulsive-thrust losses were calculated, and the results were converted into aircraft payload penalty.—(27.1 × 32.2.3).

Effect of prior air force overtemperature operation on life of J47 buckets evaluated in a sea-level cyclic engine test. R. A. Signorelli et al. N.A.C.A. T.N. 4263. *April* 1958.
Buckets of S-816 alloy over-temperatured in service operation were evaluated in laboratory and engine tests. Some buckets were tested in the over-temperatured condition while others were heat-treated prior to testing.—(27.1).

Application of a high-temperature static strain gage to the measurement of thermal stresses in a turbine stator vane. R. H. Kemp et al. N.A.C.A. T.N. 4215. *March* 1958.
Static high temperature resistance-wire strain gauges were used to measure the thermal stresses in a turbine stator vane that was subjected to a temperature variation simulating engine operating conditions. The measured values, in general, compared favourably with analytically determined stresses.—(27.1).

Acoustic, thrust, and drag characteristics of several full-scale noise suppressors for turbojet engines. C. C. Ciepluch et al. N.A.C.A. T.N. 4261. *April* 1958.
An experimental investigation was conducted with an engine in the 10,000 lb. thrust class. The acoustic study was made with an outdoor thrust stand. Acoustic data are presented in terms of sound directionality, spectrum, and sound power. Aerodynamic properties of the suppressors were evaluated over a range of Mach numbers up to 0·5 in an altitude wind tunnel. —(27.1 × 32.2.3).

Propellant vaporization as a criterion for rocket engine design; relation between percentage of propellant vaporized and engine performance. M. F. Heidmann and R. J. Priem. N.A.C.A. T.N. 4219. *March* 1958.
A method is presented for evaluating experimental rocket-engine performance in terms of the percentage of liquid propellant vaporised. The method is based on a hypothesis that rocket performance is limited by the rate of vaporisation of the propellants. An expression relating characteristic exhaust velocity to the percentages of oxidant and fuel vaporised and burned is given. The analysis is applied to the hydrogen-fluorine, hydrogen-oxygen, ammonia-fluorine, and JP-4 - oxygen propellant combinations.—(27.3 × 34.1.1).

Preliminary survey of propulsion using chemical energy stored in the upper atmosphere. L. V. Baldwin and P. L. Blackshear. N.A.C.A. T.N. 4267. *May* 1958.
Ram-jet cycles that use the chemical energy of dissociated oxygen for propulsion in the ionosphere are presented. After a review of the properties and compositions of the upper atmosphere, the external drag, recombination kinetics, and aerodynamic-heating problems of an orbiting ram-jet are analysed.—(27.4).

PROPELLERS

An investigation of single- and dual-rotation propellers at positive and negative thrust, and in combination with a N.A.C.A. 1-series D-type cowling at Mach numbers up to 0·84. *R. M. Reynolds et al. N.A.C.A. Report* 1336. 1957.
The results presented include propeller force characteristics, local velocity distributions in the propeller planes, inlet pressure recoveries, and static-pressure distributions on the cowling surfaces measured for a wide range of blade angles, advance ratios, and inlet-velocity ratios for the N.A.C.A. 4-(5)(05)-041 four-blade, single-rotation propeller and the N.A.C.A. 4-(5)(05)-037 six- and eight-blade, dual-rotation propellers in various combinations with N.A.C.A. 1-series spinners, extended cylindrical spinners, and N.A.C.A. D-type spinner cowlings.—(29.6).

SCIENCE—GENERAL

See also—POWER PLANTS

*Some measurements bearing on the principle of operation of jet silencing devices. G. M. Corcos. Douglas Report No. SM-*23114. *March* 1958.
An account is given of an attempt to explain in simple terms how ejectors, corrugated nozzles and multiple nozzles modify the turbulent mixing of a jet so as to generate less aerodynamic noise. Starting from a few facts which result from dimensional analysis, from simple analytical considerations or from experiments, a hypothesis is formulated. Experiments at low speed with corrugated nozzles and ejectors are reported. Mean velocity profiles and turbulence surveys were obtained. A few secondary observations are made concerning the change of scale caused by corrugated nozzles.—(32.2.3).

The cooling of a hot surface by drops boiling in contact with it. P. Savic. N.R.C. Report M.T. 37. *April* 1958.
Photographic evidence indicates that the mode of heat transfer from a hot solid surface to a drop of water impinging on it is by nucleate boiling. A theory is developed which describes the growth and collapse of a vapour bubble in a liquid with temperature gradient. It is shown that the results of the calculations agree in some measure with observation of other workers.—(32.2).

STRUCTURES

LOADS

Study of ground-reaction forces measured during landing impacts of a large airplane. A. W. Hall et al. N.A.C.A. T.N. 4247. *May* 1958.
Tests were conducted on a large bomber aeroplane to determine the ground-reaction forces imposed on the main landing gear under actual landing conditions. Vertical, drag, and side forces are presented.—(33.1.2).

THEORY AND ANALYSIS

Analysis of stresses and deflections in a disk subjected to gyroscopic forces. M. H. Hirschberg and A. Mendelson. N.A.C.A. T.N. 4218. *March* 1958.
The differential equations governing the stresses and deflections of a disc of variable thickness subjected to gyroscopic loading are derived. For a disc of constant thickness, solutions are obtained by a finite-difference method for a wide range of disc geometry and loading.—(33.2.4.5).

Stability of cylindrical and conical shells of circular cross section, with simultaneous action of axial compression and external normal pressure. Kh. M. Mushtari and A. V. Sachenkov. N.A.C.A. T.M. 1433. *April* 1958.
The expressions for the upper limit of critical loads are presented for cylindrical and conical shells under the combined action of uniform compression and isotropic external normal pressure. The differential equations for neutral equilibrium are solved for conical shells with simply supported boundaries. These solutions are used to obtain approximate expressions for the determination of the critical external pressure with simultaneous longitudinal compression.—(33.2.4.3.6).

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